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THE

AMERICAN JOURNAL

OF THE MEDICAL SCIENCES

FOR JULY 1872.

ART. I.—*On "Carbolic Acid," its Composition, Properties, Uses in Surgery and as an Internal Remedy.* By J. H. BILL, M.D., Surgeon U. S. Army.

ATTENTION was first called to carbolic acid, as a remedy in medicine, by Lemaire, in 1860,[1] and more fully in 1863, in a work which reached a second edition in 1865.[2]

Lister, in 1866, apparently without knowledge of Lemaire's labours, commenced a course of untiring clinical experimentation, which he has continued to this day, founding what he calls the antiseptic system. His first results were announced in the *Lancet* in the spring of 1867.[3]

Recently, A. E. Sansom, M.D., Lond., has published a work on the "Antiseptic System," treating of carbolic acid as an antiseptic.[4]

Dr. Angus Smith has also published a work on "Disinfectants,"[5] to which we wish to acknowledge our indebtedness for many facts. The three authors first named unite in the effort to make carbolic acid chaperon the germ theory of disease. We think this endeavour is the cause of that opposition which Lister's practice has encountered from certain of his contemporaries, an opposition founded on prejudice, and almost passing into strife.

Until a very recent period, the chemistry of this body had been imper-

[1] Moniteur des Scs. Méd., 1860. Moniteur Scientifique, Oct. 1862.

[2] De l'Acide Phénique de son action, sur les vegetaux, les animaux, les ferments, les renins, etc. etc., par le Docteur Jules Lemaire, 2d edition, Paris, 1865.

[3] Lancet, March, 1867; July, 1867; February, 1868; June, 1869.

[4] The Antiseptic System; a Treatise on Carbolic Acid and its Compounds, by Arthur Ernest Sansom, M.D. Lond. London, 1871.

[5] Disinfectants and Disinfection, by Angus Smith, M.D. Edinburgh, 1869.

fectly known, even to chemists. This also has restricted its use in medicine. We know of a paper which appeared not long ago in a prominent chemical journal, in which the writer—a chemist—recommends carbolic acid as a styptic, asserting that, as all *acids* are styptics, carbolic acid must be a styptic—an extreme case to be sure, but showing that a body shrouded in mystery is liable to become the subject of marvellous tales. It has been in pretty general use for the past five or six years, although we think, in a rather hap-hazard way, with no design besides the benefiting a wound, and no belief in any property beyond a mysterious eutrophy. Can we doubt that it has been the scapegoat of much unmerited blame, possibly of undeserved praise?

On the whole, the reports are in its favour, and it is gradually but surely gaining the favour of the profession, especially as a local application.

Before discussing the physiological or therapeutical bearing of the subject, let us pass in review the chemistry of carbol, as we beg leave to call it, and for which we hope presently to show a good reason.

Carbol was discovered in 1834, by Runge.[1] As it seemed to form salts with bases, he called it carbolic acid. Laurent[2] first obtained it pure, calling it phenylic hydrate. Gerhardt[3] subsequently proposed the name phenol, and Berthelot[4] extended this designation to the class of which carbol is a member. Carbol was long confounded with creasote (wood-tar oil), which is an uncertain mixture or solution of carbol, cressol, creasol, veratrol, etc., with several ethers, etc. The chemistry of carbol has been particularly elucidated by the labours of Cahours,[5] Hlasiwetz,[6] Hoffman,[7] Canizzaro,[8] Fittig and Tolens,[9] Calvert,[10] Kolbe and Lauterman,[11] and Kekulé [12]

Carbol crystallizes in needles of 1.066 sp. gr., melting at 34° C., boiling at 184° C., dissolving in twenty parts of water at 20° C., and in all proportions in alcohols, ethers, oils, etc. When heated and ignited, it burns with a yellow flame. In dilute solution, its taste is sweet, and excessively impressive. Whilst its odour in dry air is feeble and not fugitive, in moist

[1] Poggendorf's Annalen, xxxi., xxxii.
[2] Annal. Chemie und Pharmac., xliii., xlviii.
[3] Annal. Chemie und Pharmac., xlv.
[4] Chemie Organique, i. p. 466.
[5] Annal. der Chemie und Pharmac., lxxiv., lxxvii., lxix., xli.
[6] Annal. der Chemie und Pharmac., cx., cxxxix.
[7] Annal. der Chemie und Pharmac., lxxvi., ciii.
[8] Annal. der Chemie und Pharmac., xc.
[9] Annal. der Chemie und Pharmac., cxxxi., cxxxiii.
[10] Quarterly Journal Chem. Society, xviii. 66.
[11] Jahresbericht, 1869–1870.
[12] Lehrbuch der Organischen Chemie, vol. ii. p. 521 to vol. iii. p. 250, the latest and best account of carbol and the aromatic series.

air the odour is strong and evanescent. The crystals are soluble in twenty parts of water, remaining liquid at 20° C.

The formula of carbol is C_6H_5OH.[1]

We know of no body with which carbol unites without change, a change so far as we know confined to a loss of its hydrogen or oxygen, and a replacement of these atom for atom by some other body. This exchange results in the formation of two classes of compounds, according as the hydrogen of the water residue OH, or as the hydrogen of the carbon nucleus C_6H_5, is substituted.

As a rule, the compounds formed by substitutions occurring in the water residue OH are unstable, carbol being easily reproduced from them, in some cases by physical means alone. When carbol and potassium react, we have an instance of the substitution of the hydrogen of the water residue by potassium. Thus C_6H_5OH+K becomes C_6H_5OK+H. This compound is very unstable, even carbonic acid will break it up. In the same way might have been substituted an alcohol radical; thus a body resembling an acid ether (*e. g.*, acetic ether) would have arisen. This hydrogen can also be replaced by the mineral acids, such as phosphoric—carbol—phosphoric acid arising. But all of these psuedo-salts, ethers, acids, etc., are unstable. The oxygen of the water residue is replaceable by sulphur, and phosphorus by double decomposition. It is removable without decomposition, as when carbol vapour is passed over red-hot zinc, benzine resulting; thus $Zn+C_6H_5OH$ becomes C_6H_5H+ZnO.

When the hydrogen of the carbon nucleus C_6H_5 is replaced by another body, the compound formed possesses tolerable stability. Thus by the action of chlorine on carbol or on its derivatives, all five atoms of hydrogen of the carbon nucleus C_6H_5 may be successively replaced by chlorine, mono, bi, tri, tetra, and quint chloro-carbols resulting. In the same way, the acid residue NO_2 can replace one, two, or three of these hydrogen atoms, forming mono, bi, and tri nitro-carbols (the last being picric acid). The acid residues SO_3H, and CO_2H (in the latter case salicylic acid being formed) may be similarly substituted. So also may the radicals of the alcohols, as methyl.

Carbol has a hydrate discovered by Calvert, and having the formula $C_6H_5O,H,\frac{1}{2}(H_2O)$. It may be prepared by adding four parts of carbol to one of water, and exposing it to a cold of 4° C. It crystallizes in six-sided prisms; we know of no other differences between it and carbol itself. When passed through a red-hot tube, carbol vapour is in part decomposed, a little naphthaline resulting. The statement of the text-books, and copied by Lemaire (*L'Acide Phénique*), that carbol heated with ammonia in a sealed tube is converted into aniline, has been recently shown by Berthelot to be incorrect.

[1] Throughout this paper, the unitary notation is used, oxygen = 16, and is biatomic.

As yet, we have no means of estimating carbol. Its specific gravity, 1.066, is so nearly that of water, and its strongest aqueous solutions (5 per cent.) contain so little of it, that we cannot estimate it, as we do alcohol, by the specific gravity.

To detect carbol, Berthelot's is the best test. Thus, the liquid should be nearly colourless, free from aniline, or any body on which Labarraque's liquid will act. It should be warmed, made alkaline with a few drops of ammonia water, and then receive one-twentieth its bulk of the "Liq. Sodæ Chlor.," U. S. P. If the quantity of carbol present is very small after perhaps an hour, the liquid will be found faintly green, changing in twenty-four hours to a deep blue. The liquid becomes blue at once if a large amount of carbol is present. This blue passes into brown, and a brown-black precipitate falls. This test is characteristic. By it the one-hundredth of one per cent. of carbol can be recognized. The sense of taste, however, can also appreciate this dilution. When a ferric salt is added to carbol, a violet coloration occurs. The test is not characteristic; cressol, salicylol, saligenin, pyrocatechin, all allied to carbol, react similarly. The gases evolved by the action of chlorohydric acid on potassic chlorate convert carbol into tri-chlor-carbol, the odour of which is so strong and enduring that this reaction might serve as a test for carbol, if sure that chinon, salicin, etc., were absent. We are thus particular about the tests for carbol, because writers on this subject content themselves with declaring that they have readily detected carbol in the breath, urine, sweat, etc., without particularizing the method, and others fail to verify their results. We can confirm the statement of Lemaire (*L'Acide Phénique*, p. 118) that carbol does not interfere with the reaction by which oil of mustard is developed from myronic acid, nor by which bitter almond oil is developed from amygdaline by emulsin, provided 5 per cent. (or less) solutions be used. But carbol liquefied by a little heat and moisture retards the appearance of these oils for several days. We also found, as stated by Lemaire, that carbol does not interfere to prevent the conversion of starch into sugar by diastase. Lemaire believes that carbol does not act on pepsin, from the fact that dogs, to which he gave cheese, albumen, etc., mixed with carbol, digested it well. Our experiments made with infusion of rennet demonstrated that carbol did not interfere with the curdling of casein until 3 per cent. solutions were used.[1] Moreover, we have not observed, after exhibiting it remedially in doses of six grains, any interference with digestion.

Lemaire states that bodies affected with carbol readily part with it on exposure to air, and he uses this explanation to account for the fact that

[1] We know it is not proven that the curdling power of rennet is the same as the digesting power of pepsin. It is probable, however, that the curdling power is a property added to the ordinary digesting power of pepsin in young animals.

bodies like albumen, etc., after being carbolized, if exposed to the air, putrefy from loss of carbol (*De l'Acide Phénique*, p. 17, and note on p. 40). Our observations are not consistent with this explanation.

Most organic bodies retain traces of carbol sufficient to prevent putrefaction for a long time. In one case, some albumen coagulated by carbol and then exposed in an open vessel during a warm, but dry summer, at the end of four months still retained carbol, which it parted with only to warm water. Urea tenaciously holds carbol for a long while, so does gelatine and many other organic bodies. The fact is, the volatility of carbol depends greatly on the humidity of the air. Carbol possesses a great affinity for water, and the power of air to absorb it and carry it off depends, in a great degree, on the amount of moisture present. The neglect of this circumstance, and failure to correct the experiments, render the tabular statements of Sansom (at p. 14), "On the Volatility of Carbolic Acid," valueless.

The gases given off by the action of sulphuric hydride on barium peroxide, and on potassic permanganate, colour carbol solutions crimson. Sulphurous gas produces a yellow coloration. Gently heated with excess of ammoniac carbonate, a deep indigo-blue results. In short, the most striking chemical property of carbol is the variety and brilliancy of the colorations resulting in its reactions with other bodies.

For a long time, carbol was ranked among the acids. It seemed to form salts with bases. This opinion is still held both by Lemaire, *loc. cit.*, p. 41, and by Sansom, p. 65. Calvert,[1] however, whose results have been verified by Romei, has shown that when carbol is added to solution of potash, it dissolves, not in proportion to the amount of base present, which it should do had a salt been formed, *i. e.*, if carbol had played the role of an acid. We conclude, therefore, that carbol is not an acid.

Laurent,[2] at one time, regarded carbol as an aldehyde, assigning to its formula the construction $C_{12} H_6 O_2$ (old notation). But carbol is not an aldehyde, for it refuses to unite with acid sulphites and with aniline. All aldehydes unite with the former, and most unite with the latter.

The prevailing opinion, until very recently, has been that carbol is an alcohol—the hydrated oxide of phenyl; and some, whilst being driven from this opinion, still hold that it is a secondary alcohol like the isopropylic. Comparing it with wood spirit, we have $\left.\begin{matrix} C_6 H_5 \\ H \end{matrix}\right\} O$ $\left.\begin{matrix} CH_3 \\ H \end{matrix}\right\} O$. Propyl. Alcohol (carbol), Methyl. Alcohol (Wood Sp.)

This formula-making of chemists is apt to mislead. It is merely guessing that a body whose elements are known and numbered has these elements arranged in a particular manner; and if this arrangement of elements is like that of any known class of bodies, we say (*i. e.*, we guess) the substance

[1] Quarterly Journ. Chem. Soc (2) vol. iii. p. 67.

[2] Révue Scientifique, vol. xiv. p. 341.

under consideration belongs to this class. In the case of carbol, we have guessed that it should be classed among the alcohols. It is an endeavour to represent probabilities which can become certainties only by the concurrence of known and, perhaps, at the time, unknown facts. When chemists say carbol is an alcohol, they really mean to say, Is carbol an alcohol? Its atoms may be arranged like the atoms of the alcohols, but are they so arranged? An answer to these questions can only be obtained from a study of the decompositions of carbol, and from a consideration of its relations to other bodies.

The only evidence for supposing carbol an alcohol is afforded by the fact that the hydrogen of the water residue can be replaced by a metal, or a radical, as we have seen. On the other hand, we find—

(1.) An oxidizing agent fails to convert carbol into a body like aldehyde, whence, by further oxidation and dehydration, an acid arises.

When an alcohol is so oxidized, first an aldehyde, and then an acid, is produced.

(2.) When chlorine acts on carbol, carbol chlorinated compounds arise.

When chlorine acts on alcohol, aldehyde is produced.

(3.) Nitric acid forms nitro-carbols (*e. g.*, picric acid), NO_2 being substituted for H of the *carbon nucleus* C_6H_5.

Nitric acid, with alcohol, forms ethers, NO_2 being substituted for H of the *water residue* OH.

(4.) Sulphuric acid forms, with carbol, carbol sulphuric acid, not convertible into ether.

With alcohol, it forms sulpho-vinic acid, convertible into ether.

(5.) The oxygen of the water residue OH of carbol, as of alcohols, is replaceable by phosphorus-chloride, bromide, etc., but with different results as respects the products arising. In the case of an alcohol, true chlorides, bromides, etc., of the alcohol radical arise, from which the alcohol can be readily reproduced, and which show double decomposition with water, alkalies, and acids. Carbol, on the other hand, when treated with phosphorus-chloride, yields chloro-benzine, from which carbol cannot be reproduced, which does not suffer double decomposition with water, alkalies, and acids, and which is not, therefore, a true organic chloride, like the chloride of the alcohol series.

A crowning objection to the hypothesis is this: The members of the carbol sereis (save carbol itself) have their atoms arranged in two different ways. When arranged in one way, they have the properties of carbol. In the other way, they have the properties of an alcohol. Thus the group of atoms C_7H_8O has been found to have two different arrangements. When arranged in one way, we have a true homologue of carbol—cressol—a body very like carbol in all its properties. Arranged in another way, we have benzilic alcohol—a true alcohol. A more correct statement is

that the group of atoms C_7H_8O sometimes plays the role of a body like carbol, sometimes that of an alcohol; and in the latter case, we infer that its atoms must be differently arranged from what they are in the former. Or more fully stated, benzilic alcohol owes its properties to the fact that its atoms are arranged on the general plan of the alcohols. Cressol having the same composition as benzilic alcohol, has quite different properties. We infer, therefore, that its atoms must be arranged differently from the arrangement of the atoms of benzilic alcohol, and not on the plan of the alcohols in general, and hence cressol is not an alcohol. If cressol, and phlorol, etc., homologues of carbol, are not alcohols, carbol itself cannot be an alcohol. It has been replied to this that cressol (and carbol) is a secondary alcohol; *i. e.*, an alcohol containing the same atoms as the primary alcohol, but having them differently arranged—such an arrangement, in short, that on being oxidized it yields acetone, instead of aldehyde. Now, in the first place, cressol on being oxidized yields no body like acetone. Carbol, besides, not only yields no body like acetone on oxidation, but it is oxidized with great difficulty. It may also be recollected that the very existence of secondary alcohols is hardly established. We know as yet of but one secondary monatomic alcohol—the isopropylic—and from this one chemistry hardly can draw the data to establish the properties of a class. We conclude, then, that the hypothesis that carbol is an alcohol is inconsistent with known facts.

What, then, is carbol, and to what class do its analogies assign it? If not an acid, an aldehyde, nor an alcohol, what is it? Do we know of any class of bodies possessing the chemical relations of carbol? We do not. Scientific law, then, requires us to rank carbol by itself, to admit, at least for the present, the existence of a separate class of bodies of which it is the lowest member, and allows us, with Berthelot, generically to call this class—the Phenols.

There is reason for regarding carbol as benzene, in which one atom of hydrogen has been replaced by the water-residue OH. Thus:

$$\underset{\text{Benzene.}}{C_6H_5,H} + OH \text{ becomes } \underset{\text{Carbol.}}{C_6H_5,OH} + H.$$

In the same manner the homologues of carbol (cressol, etc.) arise from the homologues of benzene (toluene, etc.). Hence we say that the Phenol series arises from the Benzin series by substituting OH for H. We may notice, too, that it is possible in the Benzin series to replace two and even three atoms of H by two and three atoms of OH. So arise the biatomic and the triatomic Phenols.[1] These relations are most clearly set forth in the table below.

[1] It will now appear why we prefer the word carbol to designate carbolic acid. In the first place, it is non-committal, involving no hypothesis. Then, it comes

Table of Benzins and Phenols.

	Benzins.	Phenols.		
		Monatomic.	Biatomic.	Triatomic.
1	Benzene, $C_6H_5.H$.	Carbol, $C_6H_5.OH$.	Pyro-catechin, Resorcin, Hydrochinon, $C_6H_4(OH)_2$.	Pyro-gallol (pyrogallic acid), Frangaline, Phloro-glucine, $C_6H_3(OH)_3$.
2	Toluene, $C_7H_7.H$.	Cressol, $C_7H_6.OH$.	Homo-pyro-catechin, Orcin, Guajacol, $C_7H_6.(OH)_2$.	
3	Xylene, $C_8H_9.H$.	Phlorol (Xylol)? $C_8H_9.OH$.	Beta-orcin, Veratrol, Kreosol, $C_8H_8(OH)_2$.	
4	Cumene, $C_9H_{11}.H$.	Unknown.		
5	Cymene, $C_{10}H_{13}.H$.	Thymol, $C_{10}H_{13}.OH$.	Hydro-thymo-chinon? $C_{10}H_{12}(OH)_2$.[1]	
6	Unknown(?) $C_{11}H_{15}.H$.			
7	Unknown, $C_{12}H_{17}.H$.		Thymoilol, $C_{12}H_{16}(OH)_2$.	

Pyrocatechin and pyrogallic acid (properly, pyrogallol) have long been known, but only recently properly classified. Cressol is associated with carbol, and possesses all its properties, and according to Angus Smith (*Disinfectants*, p. 90), it is only a little inferior to carbolic acid in disinfecting power. It is supposed by Gladstone to be convertible into carbol by simple contact with zinc, or calcium chlorides.[2]

From orcin comes the litmus of commerce.

Guajacol (found in guaiac resin); veratrol (in seeds of *Veratrum sabadilla*), and kreosol, together with cressol and carbol, constitute the bulk of wood creasote.

very near to the word originally proposed by Runge. If we use the word phenol (which has been proposed in lieu of carbolic acid) to designate the class, we shall need some other word to specify the individual. Carbol is a word constructed like the other phenols, as cressol, phlorol, etc.; it is euphonic, easily written, not liable especially in prescribing to be mistaken for anything else, readily convertible into a verb, or adjective, and not yet appropriated.

[1] Carstanjen in Journ. für Praktische Chem., No. 2, 1871.

[2] Chemical News, ii. 98.

Thymol, found in oil of thyme, has been used in surgery successfully as a substitute for carbol.

The whole truth does not appear in the above table. The formula of toluene we have represented thus: C_7H_7,H. It may be arranged thus: C_6H_5,CH_3. If we take the latter representation as a true expression, toluene is benzene C_6H_5,H in which H has been replaced by methyl CH_3. *Toluene was thus made in* 1864 *by Fittig & Tollens* (*Annal. Chem. Pharm.*, 131, p. 308). Thus: when mono-brom benzene is added to methyl bromide, and the mixture affected with sodium, bromide of sodium is formed, whilst benzene and methyl, meeting in the nascent state, unite to form methyl-benzene identical with the toluene found in coal tar or derived from balsam. Toluene is thus $C_6H_4\left\{\begin{matrix}H\\CH_3\end{matrix}\right.$, and toluol $C_6H_4\left\}\begin{matrix}OH\\CH_3\end{matrix}\right.$.

This process is of general applicability, and its application has thrown much light on all the bodies composing the aromatic series. It is the most interesting synthesis since Frankland first made ethyl. Other alcohol radicals, as well as methyl, may be used. Thus, if ethyl bromide is employed in the above process, xylene (phlorene) results, and from this phlorol (identical with the phlorol obtained from baric phloretinate by Hlasiwetz) is derived. By substituting amyl bromide for methyl bromide, Fittig & Tollens made amyl-benzene, which is No. 6 of the table, and marked unknown. They have shown that there are at least four isomeric bodies derivable from benzene having the composition of No. 5 of the table. Thus, tetra-methyl-benzene = $C_6H_2,(CH_3)_4$; di-methyl-ethyl-benzene = $C_6H_2,(CH_3)_2,C_2H_5$; methyl-propyl-benzene = $C_6H_4(CH_3),C_3H_7$ (cymene), and butyl-benzene = C_6H_5,C_4H_9.

In commerce, carbol is always obtained from coal-tar, in which it is found associated with cressol, aniline, picoline, etc. The plan is to distil coal-tar, collecting the distillate of 150°–200° C., agitating this with soda lye, which, dissolving the carbol, cressol, etc., rises to the surface, and is removed, then decomposed with a mineral acid, and again distilled. The distillate of 184° C. is carbol of commerce. The details of the process, on which so much depends, each manufacturer keeps to himself, such secrets being stock in trade.[1] Carbol may be also obtained by distilling salicylic acid. This carbol is less deliquescent than the carbol of tar. Although carbol has not been made by direct oxidation of benzene, it can be obtained indirectly from that substance; for, when sulpho-benzene is fused with potash, potassic sulphite and carbol result. Berthelot has made the interesting observation that the vapours of alcohol, or acetic acid, passing through a red-hot tube, are in part converted into carbol.

It is pretty generally accepted that carbol precipitates albumen, and this supposed property of carbol has even been proposed for employment as a

[1] Consult Crace Calvert in Lancet for March, 1868.

test for albumen in urine and the like, in place of nitric acid. Our results are opposed to this view. Sansom, too, declares (p. 11) that its power in this respect has been overestimated.

When the albumen of egg or of serum is mixed with melted carbol, or even with 5 per cent. solution of carbol, the albumen is at once curdled, exactly as if strong alcohol had been added. The curds may be washed free from carbol, and will then putrefy under favourable conditions, just as any other coagulated albumen. Globulin behaves itself like albumen. A solution of albumen or globulin (from the crystalline lens), 1 per cent. strong, is not coagulated by any amount of a 1 per cent. or 2 per cent. carbol solution. A 3 per cent. carbol solution renders it opalescent, an opalescence which vanishes if more albumen solution is added. Not until a 5 per cent. solution of carbol is added does any coagulation take place. In the cold, carbol seems to be without action on albumen; for no matter how much the latter is in excess of the former, it is always possible to detect carbol by Berthelot's test, just as if so much water had been added, instead of the albumen solution. We consider it established: 1st. Carbol coagulates albumen by withdrawing its water. 2d. Carbol in the cold forms no compound with albumen.

There is a singular property of carbol, not before noticed we believe, which can properly find notice here. When albumen—as coagulated egg—is gently digested with melted carbol, it dissolves, forming a light yellow syrup. By addition of water to this syrup, albumen is precipitated, whilst, by the action of cold, the excess of carbol crystallizes out, leaving a rather bland brown syrup which tastes very feebly of carbol, and gradually dries into a transparent film not acted on by water. If the syrup is boiled, the excess of carbol passes off, and the liquid on cooling gelatinizes, becoming brown. Exposed to the atmosphere this jelly becomes white, the change seeming to commence at the centre of the mass, an efflorescence of carbol (?) crystals will also be found. After a while this whiteness disappears, the mass becoming transparent again as it loses water until it dries into a hard lump looking like a piece of copal. It gives evidence of the presence of carbol six months after its first exposure to the air, and although subjected to rain, wind, sunshine, damp, heat, and cold, it neither putrefies nor moulds. Fibrine, casein, or globulin, may be substituted for albumen.

The white spot on the skin produced by creasote or carbol, has been noticed for a long time, and has been referred to a coagulation of the albumen of the skin. But this is not the whole truth. In a few minutes after the appearance of the white spot, it begins to fade, and in an hour has disappeared. The white spot is caused by a coagulation of albumen, *and* by a contraction of the capillaries of the part. The disappearance of the white spot is caused by the dilatation which succeeds the contraction of the capillaries (with, of course, more or less reddening), by the solution of the albumen coagulated in the serum which is poured out under

the skin forming a wheal, and by the power of carbol itself to render a mass of white albumen transparent by prolonged action.

By aid of heat carbol can, apparently, be made to combine with albumen. Thus: Blood serum was acted on by the reagents named, boiled, the coagula thoroughly washed, dried, and weighed.

	Grammes.
20 cc. of blood serum mixed with 20 cc. melted carbol, washed coagula	=1.460
20 cc. " " " " 5 cc. nitric acid " "	= .786
20 cc. " " " " 40 cc. 5 per cent. carbol " "	= .902
20 cc. " " " " 40 cc. 2 per cent. " "	= .857

Egg albumen was diluted with three parts water: 20 cc. contained .5565 grammes.

	Grammes.
20 cc. of albumen solution + 10 cc. 5 p. c. carbol solution evaporated dried at 200° C.	=.5960
20 cc. " " " + 20 cc. " " "	= 5825
20 cc. " " " + 30 cc. " " "	=.6210
20 cc. " " " + 60 cc. " " "	=.6250

A more dilute solution of albumen, in which 40 cc. contained .4555 grammes, yielded these results:—

	Grammes.
40 cc. weak albumen solution + 10 cc. 5 p. c. carbol solution evaporated and exposed to 200° C.	=.4680
40 cc. " " " + 20 cc. " "	=.5995
40 cc. " " " + 30 cc. " "	=.6320
40 cc. " " " + 60 cc. " "	=.5775

After these mixtures had evaporated to dryness at a temperature below 60° C. the residua were washed with water, and then dried for a long time in an oil bath at 200° C. No carbol could be detected after subjecting them to this process. These residua, so dried, presented the appearance of the dried films obtained from a solution of albumen in carbol. They readily decomposed, emitting a rotten odour.

Carbol solutions, as is well known, precipitate gelatine solutions only when these are concentrated, so that a precipitate occurring in a strong solution is at once dissolved on addition of water. The precipitation is due simply to obstruction of water. Prolonged boiling with carbol has no influence on gelatine. It neither unites with it, nor is its gelatinizing power impaired.

Carbol behaves differently with decomposing albumen or gelatine. We divided a solution of albumen into two equal parts. One part was placed in a glass-stoppered bottle, which it fitted exactly, and was placed on ice. The other portion was allowed to decompose, and in a few days was rotten. Equal bulks of carbol solution were now added to each albumen solution. After six hours' standing in closed vessels, carbol was readily enough detected (by Berthelot's test) in the sweet albumen solution, whilst it had

totally disappeared in the rotten solution, and it was not until three more portions of carbol solution (equal in bulk and strength to that first employed) had been added to the latter, that it was possible to recognize free carbol in it. Evidently, the carbol in the latter had been acted on by some body which destroyed it, at the same time, probably, combining with it. The solution of rotten albumen, after it had received its four portions of carbol, lost its disagreeable odour to some extent. It was filtered, evaporated, and encouraged to crystallize, but no crystals formed, and it dried up into a friable mass. We may add that the sweet solution of albumen, which had received one portion of carbol, for a time underwent no change. The carbol, however, gradually disappeared, and the solution no longer reacted with heat and nitric acid, showing that the albumen had been changed. The solution became rotten. The explanation seems to be that carbol preserves a definite quantity of albumen from decomposition. If an insufficiency of carbol is added to a solution, a portion of the albumen may decompose. After decomposing, the carbol can unite with it. Hence a portion of the carbol disappears. This process being repeated, occasions the gradual but finally total disappearance of carbol, and then the remainder of the albumen decomposing, we have a rotten odour. Marvellously small quantities of carbol have been credited with preserving organic matters from decomposition. Thus Lemaire, in his chapter on the applications of phenic acid to preservation of anatomical material, recommends, p. 375, 1 per cent. solutions for injection, and says if the temperature does not exceed 20° C., the cadaver can be preserved for two months. If the temperature rises, he says, that this causes volatilization of the acid, and hence putrefaction results. Our explanation would be like that offered in the case of the sweet albuminous solution. The carbol, when injected in a 1 per cent. solution, at once combines with any decomposed albuminoids which we know have power to determine putrefaction in perfectly fresh albuminoid matters in their vicinity. Thus the predisposition to decay (if we may use the expression) is taken away. But as the body putrefies little by little, the carbol, combining with the degenerate albumen little by little, disappears, and after it has all been taken up or destroyed by the decomposing albumen, the albumen which remains, and which goes on to decompose, emits the usual rotten odour. The case would be different if 5 per cent. solutions were used. In this case the preservative property of carbol would consist in its power to abstract water and coagulate albumen. It would preserve just as alcohol does. Lemaire publishes a letter, p. 298, from M. Lambert, from which it appears that the firm of Lambert & Biraben, of Montevideo, had practically solved the problem of applying carbol to the preservation of beef without injuring its quality, and at a reasonable cost. The plan is to deprive the meat of as much water, bone, etc., as possible, then to compress it by a hydraulic engine, and then to bale it in canvas soaked in tar containing 20 per cent.

of carbol. As this was six or seven years ago, and as the plan has made no commercial demand for the beef so prepared, it is probable that some defect has been found in the process as to expense, etc., or in its results as to the vendibility of its products. Carbol does not possess the qualities which fit it for the use of the anatomist, as a substitute for alcohol. It cannot always be relied on to preserve a pathological specimen. Thus we lost, by relying on carbol, a uterus, etc., with the ovary dislocated into the cul de sac. Although the specimen was thoroughly covered with a 5 per cent. carbol solution, in a tight vessel, it decomposed, and, in two weeks, was swarming with maggots; the parts were infiltrated with pus, and had not been placed in the jar immediately after removal from the body. Lemaire would explain this by the presence of the large quantity of pus. Pus contains oil in notable quantity, this oil should dissolve the carbol, and he has shown (and Sansom also) that oil greatly diminishes all the powers of carbol. Indeed, the former writer, at p. 384, quotes Gratiolet to the effect that carbol will not preserve crabs from decomposition, because these animals are possessed of a very large liver full of oleaginous material. We attributed the loss of the specimen to the excessive quantity of decomposed albuminoid matter with which the carbol was at once neutralized.

We added carbol, liquefied by heat and moisture, to butyric, valerianic, acetic, formic, and lactic acids, to the essences of apples, pears, and pine-apples, to decomposing leucine, to phosphuretted and sulphuretted waters, with and without ammonia, to propylamine, to benzene, acrolein, assafœtida, and onion-juice, to oils of cognac, citronella, peppermint, bergamot, and turpentine, and to the liquid of horrible and penetrating odour arising in the manufacture of tyrosine by the action of potash on albumen. In none of these cases was there any diminution of odour. Usually, the feeble carbol odour was quite extinguished. The following comparative experiment was made:—

1 gramme rotten	pancreas	was	mixed	with	1	per cent.	melted	carbol	(1.)
"	"	"	"	5	"	"	"		(2.)
"	"	"	"	10	"	"	"		(3.)
"	"	"	"	1	"	zinc chloride	dissolved		(4.)
"	"	"	"	5	"	"	"		(5.)
"	"	"	"	10	"	"	"		(6.)
"	"	"	"	1	"	potassic	permanganate		(7.)
"	"	"	"	5	"	"	"		(8.)
"	"	"	"	10	"	"	"		(9.)
"	"	"	"	1	"	bleaching	powder		(10.)
"	"	"	"	5	"	"	"		(11.)
"	"	"	"	10	"	"	"		(12.)

Nos. 1, 2, 3, 4, and 5 were not deodorized. Nos. 6 and 7 were nearly deodorized—8 and 9 perfectly deodorized. In 10 the odour was diminished. Nos. 11 and 12 smelt of bleaching powder only. Neither in

Lemaire's nor in Sansom's works can we find experiments similar to the above, although the latter author attributes great deodorizing power to carbol. Lemaire, however, p. 337, "Disinfection," substantially declares that carbol is not a deodorizer. Our experiments support the latter opinion.

Little is known of a definite nature respecting the disinfecting powers of carbol. The subject is an extensive one, and difficult to handle. Lemaire and Sansom content themselves with assuming that putrefaction is due to germs, carbol kills all germs, so that it *must* be a disinfectant. Thus, Sansom says, p. 20, "All disinfectants act in virtue of and in proportion to their power as septicides." Admit this, and the case for carbol is made out. Unfortunately, evidence of this kind is about all that we are likely to get. Is it not better to confess ignorance at once?

Parkes, at Netley, conducted for the British War Office a series of experiments to determine the position of disinfectants in power, cost, etc. His results are, on the whole, unfavourable to the extensive use of carbol, for though first in power, it was greatest in cost. He found that sixty grains were required to disinfect the excreta of the twenty-four hours of an individual. Lemaire "disinfected" (?) a privy containing 15,000 litres of ordure by 200 grammes of carbol (*De l'Acide Phénique*, p. 339). Parkes, in his experiments at Netley, found that whilst he was made sick (having fever, furred tongue, headache, and changed pulse) by inhaling the odours from ordinary sewage, that this did not occur if the sewage had previously been affected with carbol. This is certainly very much to the purpose, so far as it goes. Compare such an experiment, however, with the demonstration made in our own navy, that yellow fever can be exterminated by battening down hatches, and turning superheated steam into the hold. In the latter case, the result was conclusive beyond all peradventure, 1st, that the poison of yellow fever is neutralized by superheated steam, and, 2d, that superheated steam can be used for this purpose. Who would care to exchange superheated steam for carbol in a vessel infected with yellow fever?[1]

In the application of carbol to hospital disinfection (we mean the freeing

[1] Scepticism as to the disinfecting powers of carbol surely requires no apology after the spectacle of a large steamer—the Franklin—having case after case of cholera on it, to the number of a hundred (although it had touched at a port where plenty of carbol could have been obtained), and continuing to be infected after reaching New York, so that the surgeon and others died. If carbol is a disinfectant, why was it not used to stamp out the disease on its first appearance? It is hard to imagine a case more suitable for its application, or where its use would more certainly have exterminated the infection—if it is possessed of the power so unwarrantably accorded to it. Here was a chance for a crucial experiment worth more than the assertions of all the Boards of Health in the world that so "many pounds of carbolic acid have been used in the late epidemic."

a hospital of the poisons of erysipelas, puerperal fever, and the like, and not the destroying of a mere odour), but little has been done; and we fail to see how more definite conclusions can be reached than were adopted by Dr. Russell, at the Glasgow Fever Hospital, who, after a patient use of carbol as a disinfectant in the wards of that institution, declares, "As to the practical benefit of this process, I am unable to state that this or that has been the result." (*Glasgow Medical Journal*, February, 1869, p. 210, quoted in *Braithwaite* for July, 1869.)

We think that Dr. Russell's experience will be that of every one who uses carbol to "improve the air" of a hospital; and whether it would be right to experiment with this substance for the cleansing of wards from pyæmia, erysipelas, puerperal and yellow fever, etc., whilst we have such a powerful destroyer in superheated steam, belongs to ethics. It is in the application of carbol to the cleansing of wards, ships, etc., so polluted, that the solution of the question lies, and certainly there must be plenty of opportunities for testing the powers of carbol of destroying persisting contagion, in which steam is inapplicable. It is difficult to overestimate the importance of this matter. Meanwhile, however, we may experiment, and draw inferences therefrom as to the disinfecting powers of carbol. Thus, experiments on an extended scale might be made with smallpox, and vaccine virus, affecting these poisons with varied but *definite* carbol solutions, as carbol in oil, of one to twenty per cent., in glycerine, syrup, etc. Aqueous solutions of carbol, from one-half to three per cent., should be studied in this relation, and these same strengths after the carbol solutions have been acted on with sweet and with rotten albumen—this to determine whether it acts by killing organisms or by combining with those degenerate educts of the animal body which are at once the home and the food of the contagion. The poisons of glanders, hydrophobia, and even of syphilis, might afford adjunct experiments as a check upon the above.

Having settled the question for these tangible contagions, on which we can experiment so accurately and demonstrate so clearly, we might be justified in extending our conclusions to the more inscrutable contagions—cholera, typhus, and the like. At all events, pending the grand proof (to be obtained only by the successful use of carbol in practice against definite contagions), our faith in carbol as a disinfectant would rest on something like a definite foundation. As it is, we must use carbol without purpose, perhaps without confidence, feeling all the while that we may be neglecting other certainly potent disinfectants (steam, nitrous acid), merely to be in the fashion.

We have made a few experiments to determine whether carbol in aqueous solution will destroy the poison of vaccine. We powdered a healthy scab of fresh vaccine, and divided the powder into three parts, two of which were hermetically sealed in glass tubes. The other portion was affected with two per cent. solution of carbol—enough to make a syrupy

liquid. With the lymph so prepared we vaccinated eight unprotected infants. Vaccination failed in all. The second tube was then opened, and affected with one per cent. carbol solution. Three out of four of the eight upon whom the first trial was unsuccessful, were vaccinated successfully. The other tube was opened and mixed with water, and the four remaining infants vaccinated. Of these four, only one was successful. Some five per cent. carbol solution was then affected with rotten serum, in such a way that after filtration the filtrate contained two per cent. of altered and unaltered carbol. With this was mixed some of the powder of the last opened tube, and a man who bore such faint marks of vaccination as to leave the question of his safety in doubt, was inoculated. A spurious vaccination resulted, showing that the vaccine power was unaltered, but the man after all protected.

Before concluding this part of our subject, we must notice, at least briefly, the employment of carbol in rinderpest. Thus, according to Crookes, certain farms in the track of the plague enjoyed immunity by using vapour of carbol disseminated throughout the stable. By way of experiment, a healthy beast was placed near one affected with the disease—the atmosphere of the stable being charged with vapour of carbol *and* of sulphurous acid gas. The healthy beast remained healthy for a month. A second experiment was not so favourable, for the sound beast died on the thirteenth day of the exposure. We may remark here that the interpolation of the sulphurous acid gas was improper, if it was intended to make a fair experiment on the powers of carbol. Again: he records that the disease was checked on a farm where it was raging in its usual course, by the free use of carbol *and* sulphurous acid gas. On another farm, the cattle having been divided into two lots, one lot, forty-five in number, was placed in stables treated with carbol *and* sulphurous acid gas. The other lot, twenty-eight in number, was placed in stables not so treated. The disease was accidentally brought by inoculation into both lots. In the first-mentioned lot (protected), only those individuals who were inoculated took the disease. In the second lot, all took the disease, and all died. Mr. Wm. Hope, who has had great experience in the disease, says, of a herd of two hundred and sixty cows, treated on the "carbolic acid plan" (by carbol and sulphurous acid gas?) at Crookes's suggestion, that he saved every one of which he took charge, whilst those not under his care died or were slaughtered. (Sansom, *Antiseptic System*, p. 132, quoted from *Chemical News* for Oct. 1870.) We cannot attach the value accorded by Sansom to these observations, as regards the disinfecting power of carbol; and certainly the fact of employment of sulphurous acid gas shows that Crookes did not place unlimited confidence in the powers of carbol.

Cases are recorded by a French Commission appointed by the Minister of Agriculture, of the successful use *internally* of carbol, in charbon, and furuncular epidemic. (See summary in *Lancet* for May, 1869, p. 276.)

We stand, therefore, in about the same position as in 1863, when Lemaire declared that if epidemics are caused by living germs, phenic acid should prove their destruction, cholera and other epidemics thus finding in the acid a sure cure. (*De l'Acide Phênique*, p. 362.)

Of all the elemental forms, pus and lymph are the most susceptible of changes produced by carbol. Pus is precipitated readily even by one-half per cent. solution of carbol. When a drop of mixture of one per cent. carbol solution and pus is examined under a quarter-inch objective, it will be found that all cells are destroyed, their contents uniting with the carbol to form gelatinous islets, which float in a yellowish liquid. These islets are studded with granular matter. If the microscopic stage is inclined, these islets elongate, and present the appearance of ganglionic cells under a high power. If tested chemically, it will be found that the carbol has disappeared. If excess of carbol (as demonstrated by chemical tests) be added, the islets dissolve, save the granules, and we find simply a yellow liquid with granules floating in it. With pus, as found in wounds, carbol at once unites, and the two bodies dry up into a hard oily crust.

It has been stated that carbol is without action on blood corpuscles. (*Lemaire Acide Phênique*, p. 100.) Lemaire, however, does not seem to have examined the action very carefully or out of the body. The elliptical cell of birds is best adapted for examination. Thus in the blood of the common fowl, in a very thin clot under a quarter-inch, we shall find a quantity of rice-shaped cells each with a black spot in its centre. If a little 1 per cent. carbol be placed on the slide just touching the clot so as to make the action of the carbol as gradual as possible, in the course of the carbol current we will first notice a few broken-up cells (not the true corpuscles) which blacken and shrivel up as the amount of carbol increases. Presently, however, we will find the true blood corpuscles attacked. They become smaller and rounder, darker, and at last polygonal. At first they occupied the whole field, now they are so shrunken (and destroyed?) that wide spaces exist between each cell, spaces filled with a glairy liquid. In some of the cells the black spot is seen to divide, and each half to take up a new position at the ends of the cell, the cell wall at the same time contracting in the middle, though never to complete fission. Finally, such cells as have not been dissolved (?) vanish by sinking out of focus, for, on lowering the objective, they will be found lying flat on the slide. Exactly these phenomena may be observed with human blood, although in a less degree. A 3 per cent. solution of carbol intensifies the action, but, at the same time, confuses it by its rapidity and by its action on the serum.

When the juice from the testicle of the common cock is examined under a quarter-inch objective, we will find (1) spindle-shaped cells, each containing an eel-like form, which by its contortions changes the shape of the cell incessantly. These are the "vesicles of evolution" of Wagner. (2)

Small spermatozoa like those of man and mammals, but smaller. (3) The eel-like bodies released from the "vesicles of evolution."[1]

A drop of this semen affected with ½ per cent. solution of carbol, applied as in the case of the blood-cells, failed to arrest the motion of (1), (2), or (3). One drop of 1 per cent. solution was also without effect, but two drops stopped all motion in (3); two drops of water had the same effect. A very small drop of 2 per cent. solution stopped motion in (3), but was without this action, at least for a time, on (1) and (2). A 4 per cent. solution did not entirely check motion in (1) nor in (2), though its activity was diminished. No. 3 became sharply outlined, as were the contents of (1); the cell of No. 1 began to contract. A drop of 5 per cent. solution checked motion in all three. Under its influence (1) became smaller, whilst (2) became greatly enlarged (two or three times). The cell walls of (1) were much more distinct, and the liquid in which the bodies floated thinner. On mammal semen (that of the cat errotically excited) carbol has a more decided effect. Even ½ per cent. solutions check motion in the spermatozoa at once. It should be remembered, however, that water has the same effect. By 4 per cent. solutions the spermatozoa were enlarged and better defined and not otherwise changed. The medical jurist may find this action of carbol of practical importance in pronouncing on supposed seminal spots.

White and yellow fibrous tissue are attacked by carbol solutions slowly if these are dilute, more rapidly if concentrated. The fibres become disintegrated, assuming a granular or perhaps a worm-eaten appearance. Still, neither white nor yellow fibrous tissue dissolves in saturated carbol, even when kept for weeks at a temperature of 40° C. On muscle fibre carbol (1 per cent.) has little effect; the striæ become more distinct in consequence of a general deepening of the colour. By 4 per cent. solutions the bundles become very dark and show a disposition to split up, and the carbol besides seems to exercise some solvent power on the ends of the bundles especially.

We have expended a great deal of time and work on the action of carbol on the nervous structures, but with poor result so far as discovering any specific chemical or mechanical action sufficient to account for the marked influence (which we shall prove) carbol exerts over the nervous system. By 1 per cent. solutions the cells and their nuclei are rendered more distinct, and this is true of the walls of the tubules. Stronger solutions destroy the cells, possibly owing to the affinity of carbol for oil. However this may be, the fluid becomes filled with oil globules and cell debris floating

[1] These observations were made on two cocks in the early spring, when the generative energy is strongest. The small spermatozoa (2) are not noticed by Wagner, at least not in his article on Semen in the *Cyclopædia of Anatomy*. We are at a loss to know what they are; only of their presence are we certain.

in a thin liquid. The white substance of the nerve tubule seems to be dissolved, and vanishes.

On milk 1 per cent. solutions are without effect, whilst 2 per cent. solutions render the microscopic field hazy. A complete coagulation of the casein, albumen, etc. occurs with 5 per cent. solutions. All aqueous solutions of carbol are without action on the globules. Carbol liquefied dilates the globules, but does not destroy them. Milk so affected has the appearance of white paint.

When a quantity of liquefied carbol is retained in contact with the skin for two or three minutes, the first noticeable phenomenon is the occurrence of the white spot. This presently becomes surrounded with a halo of congestion. Next serum commences to be effused, so that the skin is raised in a wheal. The smarting which was present at first now subsides, and in place thereof anæsthesia over that portion of the skin covered by the white spot begins. The surrounding congested parts are hyperæsthetic. This anæsthesia is at its height in fifteen or twenty minutes, and involves the whole dermal envelope down to the cellular tissue. The skin so affected may be punctured, cut, or burnt without sensation. A previous soaking of the parts in dilute acetic acid increases the power of the carbol to penetrate. As we have in a previous paper[1] shown how this power of carbol can be applied to operative surgery, we will not dwell longer on this point than to remark that a continued use of carbol as an anæsthetic in operations requiring only the incision of the skin has established our first confidence in it for this purpose. To deep-seated buboes, etc., where the inflammation and tenderness are seated chiefly in structures below the skin, it is not applicable. We think, too, there is something in this use of carbol which predisposes to a very rapid healing of the incision so made, for over and over again such incisions have healed without leaving any cicatrix. This was true, for instance, in two operations for epithelioma, in which a V-shaped piece of the lower lip was removed. In both cases pain was felt, though certainly in a lessened degree—about as much, for instance, as when ether spray is used. Solutions of carbol in glycerine have no anæsthetic, or, so far as we could discern, other effect, when applied to the skin. Solutions in oil are less powerful than aqueous solutions, and for various reasons are not advisable for producing anæsthesia, unless for the purpose of injecting a sinus or a gunshot wound, when they are much better than aqueous solutions, provided the strength is not less than 1 : 10. Solutions of carbol in acetic acid have very powerful anæsthetic effect, but the incisions heal badly, and even if the solution is applied to sound skin an abscess is very apt to result. This we state from actual observation.

The white spot gradually fades, the place becoming brown red, and the serum disappears about the same time. After a few days this brown redness

[1] Am. Journ. Med. Sciences, Oct. 1870, p. 573.

is succeeded by a tallow-like whiteness, and this is surrounded by a halo of fiery redness, like the line of demarcation in gangrene. Still the skin so affected does not gangrene, but dries up, cracks, and is pushed off by newly formed structures beneath. Then the surrounding redness fades, and in a period of from ten days to a fortnight all trace of the action of carbol has disappeared. A 5 per cent. solution, aqueous or ethereal, if confined to a spot, and prevented from evaporating, produces similar though less pronounced effects.

If we touch the wing of a bat with 5 per cent. solution of carbol, we will observe: 1st. A contraction of the capillaries and small arteries; 2d. Complete arrest of the circulation; 3d. Effusion of colouring matter, with coagulation. This effusion constitutes a solid clot on the *outside* of the vessels, for in a short time the vessels dilate again, and the circulation is resumed, whilst the clot remains. These effused products remain, at least in the human body, for a long time, undergoing apparently the same changes as we notice occurring in the case of a bruise. Finally only a feebly yellow trace remains, surrounding the spot where the carbol had been applied, but not invading it: this yellowness is permanent in some cases. The hair follicles are usually left uninjured.

Although creasote has been used for a long time as an application to burns, and even ulcers, yet to Lemaire belongs the credit of applying the chief constituent of creasote—carbol—with the definite purpose of preventing gangrene, ulcerations, etc., by killing the germs he believed were causative of these lapses of nutrition. Thus he applied it in anthrax, having discovered in the pus from that disease animalculæ, and with the intention, we infer, of killing the germs to which he attributes anthrax. (*Acide Phénique*, p. 452.) He used it in ulcerations, abscess, cancers, besides in various skin diseases, especially those of a parasitical origin. His theory stands forth in his practice against the more hidden internal disorders, as intermittent fever.

Curiously enough, shortly after the appearance of Lemaire's second edition in 1865, Lister, from similar theoretical considerations, announced substantially the same facts regarding carbol, and inaugurated what he called the antiseptic system. Lister has, perhaps, had greater opportunities for carrying out Lemaire's notions, and in doing so has developed and improved them as regards their practical applications. Could we but admit this evil agency of germs, Lemaire's and Lister's practice would follow as a necessary consequence. As it is in consequence of that adherence to this theory, the value of their observations diminish as their enthusiasm increases. Since Lister inaugurated the antiseptic system in Glasgow, he seems to have devoted himself to its perfection. He has tried every imaginable device for debarring germs from wounds, or for killing them after their entrance. Considering the advantages which we may derive from the use of carbol in wounds, which we undoubtedly owe

to Lister, as well as his constancy and consistency to his first idea, although refusing his theory, we cannot withhold, whilst accepting his practice, our small testimony of praise and admiration.

Lister publishes, in the *Lancet* for April, 1870, the result of the practice of the antiseptic system at the Glasgow Infirmary, claiming, and correctly for aught that has been disproved, a reduction of mortality, in major operations and compound fractures, from 1 in 2, as it was before the use of carbol, to 1 in 6. This reduction is due, he says, to prevention of pyæmia, and possibly hospital gangrene and erysipelas. We can readily admit that carbol reduces the chances of pyæmia (and hospital gangrene?). Lister, we think, fails to show any reduction of erysipelas. But in admitting the power of carbol to prevent pyæmia, we furnish no argument for Lister's pet theory. Pyæmia supposes pus. Carbol prevents the formation of pus. No pus, no pyæmia. This is the conclusion *we* draw from the papers referred to.

Lister's successes have been confirmed by the practice of many others. Bottini, of Novara, reports on six hundred wounds treated by carbol. He declares it prevents suppuration, and promotes cicatrization. (*Brit. Med. Journal*, June, 1867.) Lemaire noticed this effect, and records his faith in the power of carbol to prevent suppuration repeatedly; thus *De l'Acide Phénique*, p. 477; on Cancer, p. 490; Epithelioma, p. 602; Necrosis, etc. etc. Most of the medical journals for the last five years have contained papers detailing the successful use of carbol in wounds, recounting an experience usually of five or six cases each, but almost always remarking on the unique power possessed by carbol of restraining suppuration. Carbol, however, is no specific. Some have failed in its use (if we accept Lister's statement, because they have not applied it thoroughly, we submit because it is not adapted to every wound), and have rejected it in consequence. The aggressive way in which Lister brought forward carbol provoked a good deal of opposition, the surgeons of some of the London hospitals prejudging the question. Dr. Morton, of Glasgow, opposes it in a paper (an abstract of which may be found in the *Lancet* for May, 1870), citing cases of the failure of carbol to prevent suppuration, besides the occurrence of erysipelas under its use. His opinions would carry more weight if they displayed less of a partisan influence.

Time will show, if it has not yet shown, that the estimate of Mr. Wood, of King's College Hospital, London, comes nearest to the truth. He declares that, "whilst carbol did well applied to wounds in a hospital in good sanitary condition, it was without effect in averting those disasters which befall the wounded in a hospital, the air of which is vitiated." (*Lancet*, March, 1869.)

Our experience in the use of carbol has been comparatively limited, but that little has sufficed to convince us that carbol has a power possessed by no other known remedy of preventing or of arresting inflammation in most

cases. Although our success in its use has not been unfailing, we have obtained such excellent results in several apparently desperate injuries occurring in our service in St. Joseph's Hospital in this place, that we are now accustomed to go to any operation with the feeling that, whatever else may betide, we can arrest inflammation. We will not occupy space in detailing cases; suffice it to say we have used carbol in the major amputations, as of the thigh and arm, deligation of large arteries, as the carotid, extensive gunshot wounds of both thigh and leg, compound fractures of thigh, and in particular in a case in which nearly the whole of the frontal bone was lost by necrosis, the result of an accident; perhaps twenty-five or thirty cases in all. The cases in which carbol did badly were five or six in number, and of a lesser seriousness.

The question "Why does carbol sometimes fail?" presented itself to us at an early period of its employment, and our observation has satisfied us that this failure is due to causes connected with the patient's general health. The wounds of those patients who require alcohol and bark do badly under carbol. In these cases carbol not only fails to arrest, but it even seems to aggravate, the inflammation. Carbol, therefore, is an experiment in every case. Our rule for its use is, if inflammation, as manifested by suppuration, is not checked within twenty-four hours, to abandon it at once, and administer supporting internal treatment and poultices locally. We may add that most of the cases in which carbol did badly were wounds of hand or fingers, its tendency seeming to be to start inflammation up the sheath of the tendons, forming abscesses in the palm or at the wrist. Here an immediate removal of carbolized dressings, the free use of whiskey and cinchona, and poultices, brought about a favourable change. Other wounds of hand and fingers have done well under carbol.

But there is another object to be gained by the use of carbol as a surgical dressing, besides that of averting inflammation. This is the procurement of quiet and comfort to the patient by the anæsthesia resulting. Thus, in a case of necrosis of the upper jaw, attended with exquisite sensibility to instrumentation, by the judicious use of carbolized oil (1 : 7 by measure) on lint, and injected by a syringe, we finally obtained so much insensibility that the necessary operative procedures, repeated many times, were little complained of. The pain of compound fractures rapidly disappears under its use. Balls may be searched for with best advantage after the use of carbol. Thus, in a case of gunshot of the tibia (a pistol ball entering at the popliteal space ranging forward and downward) with abscess in the head of that bone, which we had trephined under ether, it was impossible to use the probe without resorting to general anæsthesia. The free use of carbol for twenty-four hours not only changed the whole aspect of the case, but allowed us to make such a free use of the probe and scoop for nearly an hour, that we finally discovered the ball half way down the cavity of the tibia, and readily scooped it out.

We have had one case at St. Joseph's Hospital in which carbol seemed to be the exciting cause of erysipelas. The case was one of ununited fracture of the tibia, and we had carried a strand of silk threads soaked in melted carbol through the ends of the bones, using carbol, by the way, at the point of entry and exit of the drill as an anæsthetic, the patient feeling little pain throughout the operation. On the third day, erysipelas commenced at both punctures, and soon had involved the whole leg and part of the thigh, so that energetic means were required to save the limb.

We lost a private patient from pyæmia whilst under carbol treatment. The case was one of adherent prepuce and urethral stricture. We had dissected off the prepuce, and dressed the wound with carbolized oil, under which it did badly; and we changed it after the third day for tartrate of iron and potash locally, and whiskey and bark internally. On this day the man had a chill. He was labouring under chill and fever at the time of the operation, and we therefore ordered quinia, which, after he had several more chills, seemed to arrest the disease. In about a week, we returned to the carbolized dressing, as the suppuration was too free, and the wound progressed pretty well, and finally healed. Before doing so, the man had more rigors, complained of pains in shoulders, then in his knees, etc. An abscess formed in his thigh as large as a hen's egg, and a deep ulcer remained, which ultimately healed under carbolized dressing.

Six weeks after the operation the scrotum began to swell, and presently was infiltrated with little abscesses, which finally determined the gangrene of the whole scrotum, and the lower part of the tissues of the abdomen. Pus was found after death in the knee and shoulder joints, and in most of the viscera. He had no urinary infiltration, and we had no doubt that death resulted from pyæmia.

In the practical application of carbol, our usual plan after an operation is to wash the wound thoroughly with 5 per cent. aqueous solution of carbol by means of an ordinary chemical wash bottle. Then cerate, or oil carbolized 1 : 10, is applied in as great excess as possible. Stitches are then put in (which may or may not be carbolized by soaking in liquefied carbol); and on the line of the wound, silk gauze soaked in carbolized collodion 1 : 10 is spread. After which the parts, especially if it be an amputation, are put away in plenty of oakum. In using carbol on a granulating surface, there is one fact of importance to be noticed. There comes a time in the healing of wounds under carbolized dressing, when it does harm. This happens at the period of commencing cicatrization. The dermal substitute will not form under the action of carbol. The strength of the carbol dressing must then be greatly reduced, or substituted by simple oil or water. The desired action of the granulations soon commences, and the cure is not delayed.

In parting with this portion of our subject, we express the conviction that if a surgeon, in using carbol as a dressing, *could* convince himself that

there are in wounds germs hostile to healing, to be successfully encountered only by the most liberal use of carbol, he would have no cause to regret his practice, whatever others might say of his theory.

It will be noticed that the cause which Lister assigns for unhealthy action in wounds is entirely objective. No one will deny that there are objective causes for unhealthiness of wounds, and germs *may* be one cause. But it is certain that in the vast majority of wounds, the cause of unhealthiness springs from within—is subjective. This is the established faith of the medical profession. Hence has arisen the classification of wounds into syphilitic, scrofulous, scorbutic. Hence the care of all surgeons before all great operations, to place their patients in as good a state of health as possible. Hence the fatality of operations in the gouty, scrofulous, and scorbutic; in the drunkard, the glutton, even in the plethoric, or in the obese. Can this experience of the past and present be all wrong? It may be asked, If Lister is right in his theory, whence come abscess of prostate, and the pyæmia sometimes attendant? Whence psoas abscess? How is it that abscess of lung tissue is so rare, considering the excessive accessibility of the lung to the entrance of the germs? Why is carbuncle of the lip more apt to end in pyæmia than almost any other accident, if pyæmia is the result of germs? Why should germs infect pus more certainly in this situation than the pus of a carbuncle on the shoulders? Why is it that chloride of zinc, aniline, and dilute sulphuric acid, all powerful septicides, have no power over inflammation like carbol? Lemaire, Liston, and Sansom, as we have before remarked, all seem quite as much interested in using carbol to prop up the germ theory as to cure disease. Pointing to the excellent results, they ask how can you explain these results more simply and conclusively than by admitting that carbol acts in these cases solely by its germ-destroying power? This is no argument, although really advanced by all three as such. It may be met and answered by silence, or it may be shown that some other theory will account for the facts in the case quite as well. To this end, and not because we attach any value to it, we offer an explanation, founded on the anæsthetic power of carbol. Our explanation, if not absolutely true, we believe to be in the *direction of the truth*, making, as it does, the causes for unhealthy action purely subjective.

The action of carbol in subduing inflammation is manifold. First, carbol breaks up all foreign organized bodies like pus, destroying their pseudo-life, and preventing the mischief which results from that life.

Secondly, carbol combines with decomposing albuminoids whose presence and contact tend to induce the decomposition of healthy albuminoids in their vicinity, thus becoming never-ceasing sources of irritation. Finally, as we have shown and others have confirmed, carbol suspends nerve irritability, producing anæsthesia, and, as Lemaire has noticed, also paralysis. It is to this property of carbol that we attribute its peculiar power over

local inflammation. Carbol renders the nerves incapable of transmitting impressions of pain, and *probably* those impressions also which are concerned in the nourishment or repair of tissue. The probability here involved is a strong one. Not only clinical experience but experiments have taught us that the nervous influence, although not essential to nutrition, "may, and even almost constantly does, influence nutrition and secretion,"[1] and that both afferent and efferent nerves of nutrition must exist just as we can prove that the same or similar nerves preside over secretion. Thus, if we touch the periphery of an afferent nerve distributed to a gland, although this nerve carries no sensation of which we are conscious—as of pain—yet that some impression is carried we cannot be doubtful, for the gland is thrown into action. But other irritations excite the gland, and among others that irritant coeval or identical with the condition which calls for the secretion or other action of the gland, and which we may call the physiological irritant. Thus dryness of the fauces is the physiological irritant which originates action in the salivary glands. The analogy from secretion to nutrition is perfect. Exactly these sequences are concerned in the nutrition and repair of every tissue. We are unconscious of them, for it is not the nerves of common sensibility (or at least their function of common sensibility) that are involved. We know that information is carried to the ganglion presiding over the nutrition of the part that certain materials or forces must be sent out to enable the cells to maintain their flagging energy or threatened integrity, or even (if there is a wound) to heal a breach. There is nothing new in all this; it is the view of Brown-Séquard and others, and we may say generally accepted by physiologists. There is a happy state of wounds, a state (as in ordinary healthy nutrition) in which the calls or requisitions for force and materials needed for repair, sent over the afferent nerves, are not exaggerated. Here no excessive supplies of force and materials are sent out in response. The repair is made without inflammation; it is the repair of immediate union. The force is not exaggerated; there is no heat, the materials are exactly equal to the needs; there is therefore no redness nor swelling, and without heat, redness, or swelling, there is no pain; in short, as we have stated, no inflammation. But usually this happy state of things does not obtain. The afferent nerves carry exaggerated reports of the mischief, which become exaggerated requisitions for force and materials. The presiding ganglion responds accordingly; the efferent nerves bear exaggerated influences to the vessels (or the nerves of the vessels), and to the cells, and we have excessive action, with the symptoms of heat, redness, swelling, and pain. We find that carbol prevents inflammation, that it prevents certain nerves from carrying impressions of pain, certain others from carrying the expressions of the will (according to observation of Lemaire). We sub-

[1] Consult Brown-Séquard's Lectures on Functional Nervous Affections, pp. 11–23.

mit, then, that it is not unreasonable to conclude that carbol prevents inflammation by checking or suspending the susceptibility of the nerves of nutrition to their irritants, and so moderating, if not altogether preventing, requisitions on the presiding ganglion, that no exaggerated force is sent forth, and no excessive action takes place.

These views are consistent with the promptness of carbol in subduing inflammation, with the circumstance that in certain cases of low or perverted vitality caused by constitutional depression, it acts badly, with the fact that it interferes with, instead of promoting, cicatrization (the parts by the time this stage has been reached being in nearly the condition of ordinary nutrition, and, therefore, instead of repression of action, requiring stimulation), and with the fact that another anæsthetic, morphia (as reputed by the best authorities), also subdues or prevents inflammation.[1]

The theoretical views of Lister have, we think, led to positive mischief in the matter of the antiseptic ligature of arteries. In *The Lancet* for June, 1869, Lister has shown that a bit of catgut or peritoneal membrane twisted into a thread, soaked for some time in carbolized oil, and tied around an artery, undergoes curious changes, and is converted finally, according to his observation, into a "living ligature," firmly encompassing the artery at the point of deligation. Lister's experiments were conducted on the calf. He does not seem to have tried ordinary catgut, and it is to be regretted, for, had he done so, and made more numerous experiments, examining the ligature in one animal at the end of two weeks, in another at the end of three weeks, and so on, he could not have failed to find what the "living ligature" really is. Lister has tied several arteries for disease on this plan, and has met with more than one imitator. Most of the cases reported have been successful, and for the iliacs in their whole course we should prefer the carbolized ligature. A case has been placed on record by Mr. Spence, which satisfies us that the plan is unsafe in all amputations, and in ligation of the carotid. In other ligations than those named, we fail to see that it has advantages over the time-honoured silk. Mr. Spence's case was a ligation of the carotid below the omo-hyoid. He followed out Lister's directions most exactly. On the day after the operation the patient, immediately after a fit of vomiting, became paralyzed, and ultimately comatose, in which state he died on the third day. It was found after death, that the ligature had broken (dissolved would be the

[1] From the peculiar action exercised by carbol on the nervous system, and especially from its power of diminishing local nerve irritability, we anticipate that its extended use in wounds will diminish the number of cases of tetanus therefrom resulting and decrease the mortality from this affliction. No one seems to be on the lookout for this result, and, indeed, it is only in tropical regions, where tetanus is such a frequent complication of wounds, that observations on this point can be made.

better expression), and had allowed the clot which had formed in the carotid to slip up into and plug the middle cerebral artery. Mr. Spence says:—

"On looking at the ligature with a pocket lens, the texture of the catgut was distinctly seen, but it was so gelatinous and pulpy that it seemed as if it were cast in gelatine, and a portion removed for microscopic examination, when placed between two slips of glass, spread out like a fluid. * * * I had had some experience in tying arteries on the human subject, and I at one time conducted a very extensive series of experiments on the effects of the ligature of arteries on the inferior animals; but I never saw a case in which the ordinary ligature of silk or linen thread yielded or slipped, and never knew a case in which the constricting circle gave way, as in this case. Under these circumstances, whilst I believe carbolized silk or linen thread may be perfectly safe, I cannot avoid the conclusion that catgut, however prepared, is liable to become altered, softened, and disintegrated by the heat and moisture of the living tissues around it, and thus allow the deligated vessel again to become permeable. I learn also that in a case of amputation of the thigh in which the femoral artery had been tied with catgut, hemorrhage occurred from the ligature slipping off." (*Lancet*, September, 1869.)

We have quoted this case at length, because it seems to us conclusive. One such case of positive disaster should outweigh a multitude of successful cases.

We have studied the formation of "living ligatures" in the comb of the cock. Our plan was to make a puncture on one side of the comb, and through this puncture to carry, by a *porte meché*, a piece of carbolized catgut two inches long folded in the middle. The ends being tucked in, the puncture was sealed up with carbolized collodion. The operation was then repeated on the other side of the comb, using plain catgut and plain collodion.

It will be understood that these two bits of gut were parallel to one another, ranged in the long axis of the comb, and were passed in introducing them from behind forwards, as far as possible, and nearly to the bird's beak, but not far enough to make a second puncture in the anterior part of the comb.

FIRST SERIES, September 6.

Carbolized gut passed along left side of comb	A	Cock No. 1.
Plain gut passed along right side of comb	B	
Carbolized gut passed along middle of comb	C	Cock No. 2.
Plain gut passed along middle of comb	D	Cock No. 3.
Carbolized gut passed along left side of comb	E	Cock No. 4.
Plain gut passed along right side of comb	F	

November 2d, two months after, cocks No. 2 and 3 were killed. The carbolized gut in No. 2 had entirely disappeared, and its position was indicated only by a black line. This black line seemed to be surrounded by a sheath-like cyst, containing a liquid having in it some nucleated cells of diverse sizes. The plain gut of No. 3 was encysted, and though weakened and even rotten, was unaltered in shape and size. December 7th, three months after the commencement of the experiment, killed No. 1 and No. 4. In No. 4 the plain gut F was rotten, but perfect in form, and save for the decomposition just as when introduced. It was encysted, and

from the cyst it was easily removed, and again replaced. In No. 1 B was like F in every respect. On the other hand, the carbolized gut of 1 and 4, A and E, had altogether vanished. The places which they had occupied in the comb were indicated by a series of black dots, evidently the remains of a black line, as in C of No. 2. In both these a sheath or cyst was formed surrounding the row of black dots, and in both *this sheath was compacted into a cord in size about equal to the catgut employed in the experiment.* Here and there this sheath inclosed a glairy matter, through the centre of which passed the line of black dots.

SECOND SERIES, December 3.

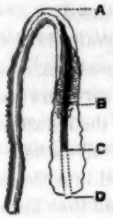
Plain gut passed down middle of comb	A	Cock No. 1.
Carbolized gut passed down middle of comb	B	Cock No. 2.
Carbolized gut passed down left side of comb	C	Cock No. 3.
Plain gut passed down right side of comb	D	Cock No. 3.
Plain gut passed down left side of comb	E	Cock No. 4.
Carbolized gut passed down right side of comb	F	Cock No. 4.

December 17, two weeks after operation, killed No. 1 and No. 2. A (plain gut) was encapsuled, softened, and covered with a little purulent lymph. B (carbolized gut) was also encapsuled, and its free ends (nearest the puncture) were softened, and looked like jelly. Along the centre of this jelly-like mass ran a black line. The rest of the gut was very much softened, but not gelatinous. The loop end least so. It was not possible to remove this from the cyst, owing to its softened condition, and also to the fact that an anastomosis of the vessels (?) seemed to pass through its substance from one side of the cyst wall to the opposite, thus uniting gut and cyst firmly together.

January 14th, nearly six weeks after operation, killed No. 4. The plain gut E was encapsuled, softened, weakened, but not rotten nor gelatinous. The carbolized gut was encapsuled and firmly fixed. It may be remarked here, that this capsule inclosed only one strand of gut. The capsule, therefore, seemed double because it was bent on itself, following the bendings of the gut, and being moulded as it were to this. The free ends (nearest the wound) of the gut were perfectly fluid, containing the black tracing in the middle. On laying open the second thirds of the capsule, it was found that both the second or middle thirds of the gut were in a diffluent state, save a central core, which, however, was quite rotten. On laying open the capsule at its last thirds, *i. e.*, at its bend, the gut was found perfect in form, but rotten. It was not easy to lift this bent third of the gut out of its sheath after the latter had been opened. This was due to an interlacing of fibres (possibly vessels) through its substance from one sheath wall to the opposite, as in B. This is shown in Fig. 1.

February 10th, nearly ten weeks after the operation, killed No. 3. The plain gut D was rotten and diffluent, and an attempt was evidently being made to remove it. The carbolized gut C had quite disappeared, and its place was occupied by the cyst which had surrounded it, shrunken into a cord: this at first sight seemed to be the gut itself. However, at the bend of this cord there was a very minute cyst filled with glairy matter, the last trace of the carbolized gut. This is shown in Fig. 2.

Fig. 1.



Cyst containing a carbolized ligature, one-half laid open: From C to D the ligature was wanting and its place indicated by a row of black dots. From C to B the ligature was diffluent, presenting a nebulous appearance. From B to A the ligature was tolerably firm, though rotten and attached to the cyst walls by fine interlacing fibres.

Fig. 2.

An old "living ligature" containing a small cyst at A laid open.

From these experiments we conclude :—

1st. Carbolized catgut is more easily decomposed by contact with living structures than non-carbolized catgut.

2d. The "living ligature" is merely the capsule which (at first thrown around the foreign body, possibly for its removal) has contracted into a firm cord.

When carbol is administered to a man in doses of six to eight grains, dissolved in a wineglassful of water, the following phenomena present themselves :—

1st. A loss of sensibility in the mouth and throat, or a feeling of numbness, as when aconite is applied to the lips: this is followed by a cooling sensation, like that produced by mint.

2d. Slight nausea, especially if the stomach is empty. This is succeeded by an uneasy feeling in the abdomen, like that felt before an attack of gravel.

3d. Slight vertigo, ringing in the ears, and partial deafness, judged by the ticking distance of a watch. This vertigo is so great, if the carbol is taken just after rising (and of course on an empty stomach), as to compel the resumption of the horizontal position.

4th. Loss of heart-beat, the pulse, according to our repeated observation, and on different individuals, losing from four to eight beats in a minute, failing also in fulness. The temperature under the tongue undergoes no noteworthy alteration.

5th. Diarrhœa. This is not invariable, and does not appear until several doses have been taken. If present it usually disappears on the third or fourth day of the continued administration of the medicine.

6th. After long-continued use of the medicine, feebleness of heart-beat, muscular debility, and loss of flesh occur.

On omitting the medicine after it has been taken for several days, flatu-

lence usually occurs, accompanied with a feeling of depression, like that felt after the stimulating effects of morphia have ceased.

After taking the medicine in doses of forty-five grains in the twenty-four hours, we entirely failed to detect it in the pulmonary exhalation by Berthelot's test, although many gallons of expired air were examined, and the observation made within a half hour after the ingestion. Lemaire states that the lungs are the chief emunctories of carbol. Thus, after taking it in quantities of a gramme a day dissolved in a thousand parts of water, he declares that he expired it all day long, and that his urine smelt of it, though feebly (p. 95); and, again, on p. 107 he declares that, according to his experiments, the greater part of phenic acid introduced into the stomach is eliminated by the lungs. We will only add, that our experiments were made with care, and with knowledge of this assertion of Lemaire. We can further say that we have never observed the odour of carbol either in the breath or in the urine of those to whom it has been administered: one-half of one per cent. added to urine communicates a distinct odour of carbol to that liquid. We suspect that Lemaire has trusted to his sense of smell rather than to a chemical test in making these assertions, in support of which he offers no proof—at least not in the work from which we have quoted. We have failed to discover it in the feces, and in the sweat after violent exercise. Our observations are, however, limited to cases in which a drachm, or less, in the twenty-four hours was administered.

The urine, according to our observations, after ingestion of thirty to forty grains of carbol in the twenty-four hours, is natural in colour and odour, and deposits, by boiling, no albumen, but a little phosphates only, so that it readily clears up on addition of an acid. But if the heat is applied for some time—and the liquid need not boil—so that it is one-third evaporated, a brownish discoloration will be observed, which speedily changes into a black precipitate, coloured grayish, however, by admixture with the large amount of phosphates precipitated at the same time. After this precipitate subsides, the urine is found clear, and the further action of heat fails to produce any unusual changes. In one case, after taking forty-five grains of carbol, the urine of the twenty-four hours yielded a grayish precipitate, which, after washing with dilute acetic acid, and drying at 100° C., weighed two hundred and eight grains. Before the blowpipe, this precipitate took fire, and burnt with a sooty flame and a singed-hair odour, leaving a black coal, which yielded chlorhydric acid, a large amount of iron, and phosphates. The former was thrown down by ammonia water in rusty flakes; the latter appeared as a microscopic deposit of triple phosphate. The gray precipitate was partially soluble in dilute hot potash solution, with an odour like that produced by the action of dilute potash solution on albuminoids in general. From this solution an acid threw down a glairy precipitate like globulin. Chlorhydric acid also

dissolved the gray precipitate in part, and yielded, on addition of an alkali, iron, phosphate, and a body containing carbon and nitrogen. Ordinary sodic phosphate was without action on the gray precipitate. Water, alcohol, and ether failed to dissolve it. We know of no substance in the body, save hæmatin, which contains at once the amount of iron and phosphates called for by this body. We made no quantitative estimation of these, but this was not needed to satisfy us that both iron and phosphates were present in large quantity. Of course the quantity of the precipitate obtained precludes the supposition that the body in question was a derivate of carbol. It was evidently an albuminoid, and from all the facts in the case, we believe it was hæmatin altered by or combined with carbol. We do not pretend to account for the impunity with which the body was deprived of two hundred grains of hæmatin *per diem*. We cannot find that any one has noticed this substance. Its appearance must not be confounded with another phenomenon long observed after ingestion, by mouth or skin, of creosote, tar, and carbol. We refer to the smoky or brown coloration observed in the urine under these circumstances, especially when from any cause the urine is ammoniacal. If such urine is placed in a tall jar, the browning will be observed to commence at the surface and proceed downwards. It seems to form *pari passu*, with the decomposition of the urea, into ammonia, and it usually renders the whole urine black-brown in three weeks. This solution has been the subject of discussion and experiment by several observers, thus: "Dr. Wallace concluded that the carbolic acid was carrying off the pigment of the red corpuscles. * * * Dr. Stevenson, of Guy's Hospital, London, showed that urine darkened by the systemic absorption of carbolic acid, did not contain more than the normal quantity of iron. He considers that the colouring matter was not derived from the blood. The most probable hypothesis is, that it comes from the *changed carbolic acid* itself. (Sansom, p. 283.) We coincide with this last opinion, and we even hazard a conjecture as to the nature of the change, and the name of the derivative. It must be obvious from analogy, in the first place, that any change which carbol undergoes in the body must be one of oxidation. As a rule, this is true of all substances, especially those whose educts appear in the urine. We must expect to find carbol transformed, by the time it reaches the kidneys, into a body containing more oxygen. Chinon, whose formula is $C_6H_4O_2$, is such a body. We have a perfect analogy to the changes we have indicated as happening to carbol, in the changes which alcohol (in part, at least) is supposed to undergo in its passage through the body. We may compare the changes of the two bodies, thus:—

$$\underset{\text{Alcohol.}}{C_2H_6O} - 2H = \underset{\text{Aldehyde.}}{C_2H_4O} + O = \underset{\text{Acetic acid.}}{C_2H_4O_2}$$

$$\underset{\text{Carbol.}}{C_6H_6O} - 2H = \underset{\text{Unknown}}{?} + O = \underset{\text{Chinon.}}{C_6H_4O_2}$$

That is, alcohol and carbol lose two atoms of hydrogen, and gain one atom of biatomic oxygen.[1]

So far then the case stands thus: There is a probability that carbol in its passage through the body is converted into chinon. We must now offer some corroborating evidence of this proposition, and we will be as brief as possible. Chinon presents the singular property of becoming reddish-brown when allowed to stand for a while in aqueous solution. This change is promoted by heat and by addition of ammonia; the brown becomes almost a black. These phenomena, as we have seen, occur in urine after carbol ingestion, and in fact they constitute the matter under discussion. In the next place we will find that certain reactions can be observed in urine after carbol ingestion—which must be due to the presence either of chinon or carbol—we throw salicine, aniline, and the rest out of the question; they are not present. Thus, after taking forty-five grains of carbol, the urine, if acted on by chlorine, or better by enchlorine of Davy (obtained by action of chlorhydric acid on potassic chlorate), yields the peculiar odour which dilute carbol or chinon solutions do under similar circumstances, and as the action of the chlorine proceeds a minute quantity of what we take for perchlor-chinon is formed, colouring the foam of the urine pale yellow. The case then seems to narrow down to the presence either of carbol or chinon. Let us show now that carbol is not present, and we will have another fact pointing to the presence of chinon. It is true that carbol cannot be detected by the direct application of Berthelot's test to urine. We have noticed that, even after the occurrence of the blue coloration indicative of carbol in a liquid, the addition of urine immediately rendered it colourless;[2] but, if we add even the one-hundredth of one per cent. of carbol to urine and distil it, we can readily detect carbol in the distillate by Berthelot's test. This plan applied to urine passed after ingestion of carbol has invariably in our hands failed to indicate carbol. We consider this fact conclusive, therefore, that carbol does not appear in the urine after its moderate or non-poisonous ingestion. The foreign body contained in such urine which is acted on by chlorine yielding perchlor-chinon, is again proved to be chinon. One more proof and we have done. We affected 600 cc. of urine with a little chlorhydric acid, just as in the usual way for estimating the quantity of uric acid in urine. Of this 200 cc. was placed in a beaker by itself, 200 cc. in another beaker con-

[1] This reaction, by which carbol is converted into chinon, we have succeeded in effecting in the laboratory by the action of potassic bi-chromate on carbol. The chinon volatilizes, and should be condensed in a tube.

[2] This phenomenon is due to uric acid. If an aqueous solution, coloured blue by addition of Labarraque's liquid and ammonia (the latter must not be in excess), be shaken with uric acid, the colour is discharged. The colour can be restored by ammonia.

taining a trace of chinon (the urine, by the way, becoming blood-red, a phenomenon observed, by several writers, in urine after carbol ingestion), 200 cc. more were placed in a third beaker containing a small quantity of one per cent. carbol solution. The beakers were then covered with paper and put away for the formation of uric acid. After twenty-four hours crystals of uric acid were first observed at the bottom of the beaker containing carbol, and twelve hours later crystals were observed in the beaker containing urine only. But at no time, not even after the other two liquids were crowded with crystals of uric acid, were any crystals observed in that beaker to which chinon was added, but in place thereof a pale muddy precipitate in small quantity was observed at the bottom of the beaker. The appearances obtained in this last case are found when, after the ingestion of carbol, we seek to discover and estimate the uric acid by addition of a little chlorhydric acid, *i. e.*, we have no uric acid in crystals (or but a trace) but a quantity of a muddy precipitate. These arguments we think are sufficient to secure at least respect for our conjecture, that chinon is found in urine after ingestion of carbol as a product of the oxidation of the latter. We forgot to mention that chinon is not poisonous to dogs in quantities of a drachm. (Wöhler and Frerich's *Annalen Phar.* 65, p. 343, quoted in Gmelin's *Handbuch der Chemie.*)

In the table below the effect of carbol ingestion on the urine is set forth; the most noticeable thing is the lack of uniformity in its action. As we remarked, when speaking of the uses of carbol, that every case is an experiment, so in the effects on the urine every case is a law unto itself.

The urinary water, the specific gravity, and the colour were unaffected.

The urea slightly increased.

The phosphates greatly increased.

The uric acid frequently could not be observed at all,[1] and always there was such a quantity of muddy deposit that its exact estimation was impossible.

The acidity was increased.

[1] Fuller has noticed this circumstance, and thinks carbol prevents *formation* of uric acid. In cases where considerable uric acid deposits had been occurring, these disappeared quickly under action of carbol. He applied his ideas unsuccessfully to the treatment of gout.

Table of Analyses showing the influence of Carbol on the Urine.

	No Carbol taken.					Carbol taken.					
	Total urine of 24 hours.	Urea of 24 hours.	Phosphoric acid of 24 hours.	Uric acid of 24 hours.	Total acidity in acetic acid.	Total urine.	Urea of 24 hours.	Phosphoric acid of 24 hours.	Uric acid of 24 hours.	Acid of urine estimated as acetic acid.	
	cc.	grms.	grms.	grms.	grms.	cc.	grms.	grms.	grms.	grms.	
Healthy man, æt. 34. 1	1370	27.04	2.70	.524	2.9	1450	31.17	2.90	.36		5
2	1980	31.68	..	.680		1600	31.20	2.86	none		6
3	1750	31.50	..	.530	2.4	1500	30.75	3.80	none	4.5	7
4	1200	..	2.52			1250	31.25	3.40	.30	5	8
						1480	38.48	3.19	.41	3.4	9
Average	1575	30.07	2.61	.578	2.6	1556	32.57	3.23	.21	4.3	
Cancer patient, No. 1, æt. 70. 1	920	22.03	1.17		Alkaline	570	16.24	1.91		Faintly acid	4
2	445	14.46	.95			875	18.67	1.78			5
3	720	16.05	1.02			930	16.20	1.50			6
Average	695	17.53	1.04			758	17.03	1.73			
Bubo patient. 1	900	23.22	1.47	.429		880	23.76	1.58	.450	4.3	3
2	1185	23.80	1.89	.300		850	22.20	1.51	.480	5.2	4
						910	24.50	1.54	.500	4.5	5
Average	1042	23.51	1.68	.354		880	23.48	1.52	.476	4.6	
Polyuria patient. 1	9850	26.80	1.90			10700	27.03	3.75			4
2	8500	29.50	2.00			8840	28.12	3.20			5
3	11670	27.04	1.97			9945	30.12	3.80			6
Average	9990	27.78	1.95			9762	28.42	3.58			
Cancer patient, No. 2, æt. 30. 1	1050	20.01	1.80	.570	2	1160	21.75	1.95	.047	3.4	4
2	1120	19.70	1.49	.585	2.3	940	21.08	2.30	.021	2.9	5
3	1010	20.50	1.60	.600	1.9	1090	20.20	2.97	none	4.0	6
						1025	22.00	2.14	.050	3.6	7
Average	1060	20.07	1.63	.585	2	1053	21.25	2.24	.029	3.5	
Healthy man. 1	1500	31.42	2.70	.530		1450	33.27	3.19	?		3
2	1430	31.99	3.11	.601		1930	33.93	2.89	?		4
Average	1465	31.70	2.90	.565		1690	33.60	3.04			

We can say but little for the success of carbol as an internal remedy, and, although we have tried it in a variety of affections, we have seen good results in only one or two. The medical journals, however, teem with cases of its successful internal administration. Scarlet fever, measles, smallpox, typhoid, and malarial fevers, and even phthisis and syphilis, have received—as it is alleged—checks from its use in this way.

In the first place, we may call attention to the cases in which Lemaire has used it. The detail of these cases occupies more than one-third of his work, but with the exception of those cases in which the carbol was used as a local agent to subdue inflammation, or to destroy a parasite of the skin, his results seem fanciful, or unimportant. Sansom has given a table of cases in which "sulpho-carbolate of soda" was used in typhoid fever.

His results, taken at the very best, are not favourable, for out of twenty-four cases he had three deaths, or a little over twelve per cent., a low, but by no means unusually low, mortality. But, on examining the table closely, it will be found that no less than nine of these cases were under the age of ten years. These should have been excluded (for typhoid, practically, has no mortality under this age), so that we get three deaths in fifteen cases—one in five,—or twenty per cent., a mortality above the average. The table then proves nothing. Pecholier, of Montpelier, reports on sixty cases of typhoid treated by (carbol?) creasote. He asserts that it hastens recovery, provided it is given in doses of three drops from the very commencement of the disease, whilst in those cases in which the treatment was commenced after the disease had made progress, it was of no avail. M. Pecholier started out with the theory that in typhoid there is present,[1] as a cause, an organized ferment to be killed. He believed that carbol would kill the ferment, and so cure the disease. Is the judgment of a man thus prejudiced to expect a particular result trustworthy, for the consideration and discrimination of results of treatment in typhoid fever, in which the mortality is so variable and dependent on so many different circumstances?

Freulich reports eight cases of inveterate intermittent with enlarged spleen, treated successfully by a total medication of four and an eighth grains of carbol! (*New York Medical Journal*, from *Wien. Med. Pres.* xii., 1867.) Drs. Barrant and Tessier, at the Mauritius, have used carbol in grain doses thrice daily in intermittents. The results compare favourably with quinia. They also employed hypoderms one grain to thirty minims. (*Lancet*, May, 1869, p. 279.)

Dr. Fuller, of St. George's Hospital, London, prefers carbol to creasote in the treatment of diarrhœa.

It has been extensively used in a pulverized form as an inhalation in phthisis, and has even acquired a popular reputation in this way. We have failed to derive the slightest benefit from it in several cases, all in the suppurative stage, beyond what the same amount of steam might have produced.

In chronic rheumatism, given in sheer desperation after the failure of every remedy, we can report five unsuccessful cases.

It is worse than useless in gonorrhœa, and seems to aggravate nasal catarrh, according to our experience.

It has been used as an injection in ascarides, but its employment is fraught with danger, as Lemaire has shown.

It is used extensively in Hebra's wards, and has been especially serviceable in psoriasis, pityriasis, and pityriasis rubra, as a *curative* agent. It

[1] Quoted from Gazette Hebdomadaire, xiii., 1869, in Half-Yearly Abstract Med. Sci. for July, 1869.

is given in pills. The dose is six to ten grains gradually increased to twenty or even to sixty grains in the day. The smoky urine after this use of carbol has not been noticed. (Quoted in *Am. Journ. Med. Sci.* for April, 1870, from *Arch. für Dermatologie und Syphilis.*) Employed externally we have never failed to afford relief to the itching of urticaria, eczema, and psoriasis, and we believe that it would not fail in true prurigo. Usually, we order a simple wash of 2 per cent. carbol solution. If, however, there are open sores, such as we find in the eczema succeeding an ulceration of the veins of the leg, then oily applications are better, and we use carbolized oil 1 : 10 on rags, with oiled silk over all. We would say more on this subject if it would render our statements of any more force. We use carbol in every skin disease where itching is complained of, and we have not seen it fail to relieve this symptom. We may state here that we have not seen any constitutional effects from carbol used for this purpose, and in the manner indicated. Its use as a parasiticide requires much caution, owing to the amount and thoroughness of the application required. It has been proved by numerous observations that carbol is really absorbed through the skin, and if thrown, *as carbol*, directly on the great nerve-centre, the effects may readily be disastrous. There is no doubt, from cases recorded by Lemaire, Sansom, and others, that grave constitutional mischief is sometimes occasioned even by its application to a limited surface, or to a small wound.

Carbol has been used in cancer, and reported well on. Dr. F. A. Burrall, of New York, has reported[1] a case of mammary schirrhus, remarkable for its successful treatment by the internal use of carbol. The diagnosis was made with care, and was assured by the concurrence of several surgeons. The tumour was ulcerated, the seat of frequent hemorrhages, and was discharging fetid pus. The patient's general state was cachectic. Carbol lotions had previously been used, but without benefit. The patient was placed upon quinia, and carbol in four-drop doses thrice daily. No change took place for two months save that the pain was mitigated. At the end of this time the tumour began to soften, the ulcers to heal, and at the expiration of six weeks she was again at her work as a washerwoman—the tumour not gone entirely but practically removed. Lemaire records a case of epithelioma healing quickly under the external use of carbol, p. 491, and a second in the practice of M. Gonel, also healing by its external use, p. 493. At p. 494 he records an unsuccessful use of the remedy in the same disease, although the pain was ameliorated, and in the same place he quotes Dr. Declat to the effect that the remedy internally and locally had benefited, if it had not cured, a case of cancer of the tongue. He records four or five other cases of cancer (scirrhus) in which carbol, externally applied, gave great relief: he does not seem to have used it internally.

[1] Medical Record for November, 1869.

We have treated four cases of cancer with the remedy.

The first in an old man from whom we had removed an epithelioma of the lower lip. The disease returned in the cicatrix. Circumstances forbade a second operation. We placed him on sixteen grains of carbol (in divided doses) a day, and kept up the treatment for forty-nine days. At the end of this period it was found that the disease had quite disappeared. In this case the treatment by carbol was continued, for precaution sake, until the man began to lose flesh and muscular strength. Tonics were then substituted. This occurred in 1869, and the man has not been since troubled with his complaint.

The second of our cases was also epithelioma. We had twice excised parts of the lower lip, the last time using carbol locally as an anæsthetic. After the second operation the parts healed almost by the first intention, so rapidly indeed that the sutures were with difficulty removed. The parts remained sound for a while, but at length became the seat of one or two hard, wart-like ulcers, the source of sharp pain. Carbol was given internally in doses of fifteen grains a day. The ulcers healed, the nodules vanished, and with them the pain. The man has had no return of his disease.

The third case was one of encephaloid of the neck, which ultimately increased to an enormous size, involving every tissue between the skull and the thorax. The man of course suffered greatly. In the earlier stages of the disease, carbol in five-grain doses, thrice daily, arrested its progress, and greatly assuaged the pain. But the loss of flesh and strength was so great under its use that it was necessary frequently to suspend its administration, and in these intervals the disease made up for lost time. Finally carbol seemed to have no effect whatever, and only the most prodigious doses of opium and chloral gave ease. The man died from the immediate effects of these two remedies (comatose) which he was allowed to use *ad libitum*. Our lack of success in our later administration of carbol in this case may have arisen from the fact that we employed glycerine as the menstruum, hoping it would be better tolerated by the stomach. Lemaire has shown that glycerine diminishes the effects of carbol.

The fourth case, rodent ulcer of the lower lip and chin, is yet under treatment, but with very promising results. The ulcer, from the size and shape of a tablespoon bowl, has contracted to the diameter of a half-dollar, and granulations still rapidly coming forward. The pain left at once, and the swelling of the glands under the jaw in a few days after the commencement of the treatment. The discharge, which smelt horribly, has disappeared altogether. This man used carbol locally, 5 per cent. solution, and internally five grains four times a day. It has not affected his health in any way as yet, although he has taken it for nearly a month.

For dispensing, carbol can be most conveniently kept in the liquid state. A very small quantity of water added to melted carbol will prevent its re-

turn to the crystalline form at ordinary temperature. A minim of carbol thus liquefied corresponds almost exactly to a grain—one fluidrachm weighs sixty-one and three-quarter grains. In our opinion this should be the officinal "carbolic acid" of the U. S. Pharmacopœia. Carbol should not be given in a stronger solution than three grains to the fluidounce of water. As before noticed, in Vienna it is given in pills. It is hardly necessary to observe that these should be freshly prepared. Possibly the albuminous jelly described on p. 26 might be found useful. It is in preparing carbol for external use that the pharmaceutist will find most need of skill. We prefer as a solvent for carbol in surgery perfectly sweet salad or lard oil. We keep an oil composed of equal parts of carbol and sweet oil as stock. This is labelled "normal carbol oil," and for external use is quite as strong as can ever be justifiable. In prescribing compounds of lesser strength we write simply Carbol ol. 1 : 10, *i. e.* two parts of normal carbol ol. and eight of sweet oil, containing, therefore, in ten parts nine parts of oil and one of carbol, by measure. If cerate is wanted all that is necessary is to substitute melted wax, free from paraffine (in which carbol is insoluble) for oil, and stir the mixture until it is stiff. Normal carbol oil is coloured red with alkanet to prevent mistakes.

Carbolized collodion should be prepared in small quantity at a time. That long kept is irritating, possibly from presence of picric acid.

Carbol can scarcely be called a poison, although several deaths from its reckless use are on record. It seems to be more obnoxious to some individuals than to others, and, as a rule, badly borne by children, just as with alcohol. Indeed, it has been noticed by some one (the authority escaping our memory) that those who have been accustomed to alcohol—as spirits—and therefore insusceptible to it, are likewise insusceptible to carbol. Two persons died from the use of carbol as a general wash to the surface of the body, in the Aston work-house at Birmingham. The crude liquefied acid was the preparation used. A third person, similarly treated, recovered, which ought to be proof that carbol is *not* a poison. (Machin, *British Medical Journal*, 1868.) Lemaire records its constitutional effects from its local application in six cases of lepra. He nearly lost a child by a very dilute injection to dislodge ascarides. We have used carbol a great deal externally, as an anæsthetic, in skin diseases, neuralgia, and the like, as well as for the preventing of inflammation in wounds, not in adults alone, but in children also, and we have not seen a solitary case where it manifested a poisonous tendency. We doubt not that the cases of the judicious use of carbol locally attended with unpleasant general symptoms were instances of idiosyncrasy. It may be admitted that the external use of carbol presents greater elements of danger than the internal; for, if the solution be strong, and extensively applied, we may have a kind of shock arising from the excessive impression made on the periphery of the nervous system, as in extensive burns. But, besides this, it is worthy of considera-

tion, if liver and lungs may not act as destroyers of carbol when taken internally, so that even excessive doses are scarcely felt at all if dilute, whilst, when applied externally, it may be carried direct to the nerve centres as carbol, and kill by direct action on these. Internally, carbol can hardly be called a poison. It is probable that a five per cent. solution would kill, but the action would be a purely mechanical one, like that of caustic potash. The five per cent. carbol solution, if diluted, would be as innocuous as dilute potash solution.

As antidotes to the caustic effects of carbol taken into the stomach, oil and glycerine have been recommended on the theoretical grounds that both these substances impair the action of carbol. It is further an advantage that one or the other of these substances is almost always at hand. We should have more confidence in rotten albumen—as, addled egg—but this might not be easily obtained. To combat the effects of carbol after its absorption, our main reliance should be on the judicious use of alcohol as a nerve stimulant.

The question of the toxic power of carbol would naturally lead us to consider the action of carbol on fungi, etc., and its power to arrest fermentation. The subject, however, is too extensive for our remaining space to admit of discussion, and, besides, such a discussion would involve questions connected with the germ theory of disease, quite as much as with carbol. This is ground we have endeavoured, when possible, to shun in the paper now concluded.

From the facts laid before the reader, we think that the conclusions are justified—

1. It is not proven that carbol is a general disinfectant.
2. It is of the greatest use to disinfect wounds.
3. It accomplishes this (*a*) by destroying pus, etc.; (*b*) by preventing inflammation.
4. Its use in wounds moderates pain.
5. Its use on the skin relieves itching, and produces an anæsthesia sufficient for minor cutting operations.
6. It seems to be of use internally, in certain cases, in scaly skin diseases, and at least as a moderator of pain in cancer.
7. It has not proved of decided use in other diseases.

FORT VANCOUVER, W. T., December 1, 1871.

ART. II.—*Clinical Observations on Pelvic Peritonitis and Peri-uterine Cellulitis.* By JAMES L. BROWN, M.D., Physician to Out-door Department of Woman's Hospital, New York; Physician to Demilt Dispensary; Assistant Physician to Strangers' Hospital, etc.

THAT the uterus and its appendages, in common with other parts of the economy, are liable to become the seat of inflammation and abscess, and that such inflammation and abscess appear to be for the most part connected with the puerperal state, are facts that have been observed and recorded as far back as the history of medical science extends. Beyond this general knowledge of a very obvious affection, however, it may safely be stated that almost nothing was known of the inflammatory disorders of the peri-uterine tissues prior to the beginning of the present century. Indeed, without much violence to historical accuracy it may be affirmed that nearly all that is now known with any precision of these ailments has been ascertained within the last thirty years. Isolated facts had, indeed, been observed and recorded by different medical writers as far back as Morgagni in the early part of the last century, but no systematic study of the peri-uterine inflammations can be said to have been undertaken before the year 1840. I am aware that Simpson and others claim for the ancients a very considerable and tolerably exact knowledge of these diseases, but I am of the opinion, that no one who will take the trouble to read the passage in Aëtius, referred to by Simpson in his article on pelvic cellulitis, will find in it a justification of the statement that it contains "a full and distinct account," or indeed any account at all, of the affections under consideration. I repeat, then, that neither in ancient nor in modern times, so far as we have any means of knowing, were these affections made the subject of special study until about the year 1840. Of the progress that has been made since that time accounts more or less complete are to be found in nearly all of the more recent treatises on diseases of women. I shall make no apology, therefore, for omitting historical details that are so readily accessible, and shall confine myself in this paper chiefly to some clinical remarks based upon the observations of 87 cases, of which I have taken notes, and upon impressions received from a considerably larger number, of which no record has been kept.

As it is not my purpose to attempt any full or extended description of the peri-uterine inflammations, the subject being altogether too large to be adequately discussed within the limits to which I am necessarily restricted in a paper of this kind, but merely to invite attention to certain points that have seemed to me to be of peculiar interest, and on which it is very desirable that the light of further clinical experience should be thrown, I shall not restrict myself to any systematic treatment of the subject, but deal with it in a somewhat desultory manner, passing lightly over many

interesting topics and dwelling upon others that are of no more real consequence perhaps, but which may have proved more interesting in my own experience, or to which I may have given more especial attention. Of a class of affections that have only so recently been made the subject of investigation it is not surprising that our knowledge should still be very imperfect; that much difference of opinion among observers should exist on many points; and that what *is* known about them should be mostly confined to those especially engaged in studying or treating the diseases to which they are allied. Believing that these affections are far more frequent than is generally supposed, being often mistaken for other conditions and consequently subjected to inappropriate treatment, and believing, moreover, that they are of great practical interest to the general practitioner as well as to those more particularly engaged in the treatment of diseases peculiar to women, I have thought that any contribution tending to throw light upon them and to lead to a better appreciation of their frequency and importance might prove interesting and perhaps profitable.

Synonyms.—The affections to which I have alluded are variously described by different authors as pelvic abscess, pelvic cellulitis, inflammation of the uterine appendages, inflammation of the broad ligaments, peri-uterine inflammation, peri-metritis, para-metritis, peri-uterine phlegmon, peri-uterine cellulitis, pelvic peritonitis, etc. Some of these expressions, it will be noticed, not only serve as names, but indicate theories. Pelvic abscess, for instance, suggests the idea of suppuration as the most noticeable feature in these affections. Inflammation of the uterine appendages is exclusive, shutting out all the inflammatory affections of the pelvis, save those which it mentions. Pelvic cellulitis and pelvic peritonitis locate the disease, each, in a particular tissue, while the use of the term peri-uterine inflammation implies a disinclination to be committed to any theory as to the precise seat of the inflammatory action. We thus find in the very nomenclature of these diseases evidences of a conflict of opinion as to their seat, their nature, and their tendencies. Passing now from the nomenclature to the definitions given by different authors, we meet with a corresponding want of agreement. Selecting from a few, only, we find, for example, the following account in Simpson's article on pelvic cellulitis:—

Definitions.—"Between the layers of the broad ligaments, between the vagina and the rectum, between the iliac muscle and the bone, and, in short, in almost every part of the pelvis, there is an abundance—a great abundance—of cellular tissue; and this disease consists of inflammation, acute or subacute, of this abounding cellular tissue."

West speaks of it as an inflammation

"which, though most frequently involving the cellular tissue between the folds of the broad ligaments, sometimes attacks that which intervenes between the womb and the adjacent viscera, or extends to that lining the pelvic walls, or even to that which lies between the outer surface of the peritoneum and the abdominal muscles. * * * The cellular tissue anywhere in the neighbourhood of the womb may be the seat of the mischief, though that contained in the folds

of the broad ligaments is attacked far more often than the same structure in any other situation."

McClintock makes the following statement:—

"In regard to its seat, it may affect almost any part of the cellular tissue adjacent to the uterus. Thus I have seen it most frequently in the broad ligament, at least so far as I could judge; in the iliac fossa; between the bladder and the abdominal parietes; between the uterus and the rectum; and in every part of the connective tissue around the neck of the womb."

Graily Hewitt defines it to be essentially an

"effusion of morbid products into the space surrounding the uterus and ovaries, and the transformations undergone by these effused matters. The actual seat of the effusion is, in most cases, the meshes of the cellular tissue surrounding the uterus, between the folds of the broad ligament and extending thence in various directions towards the pelvic walls; but it is probable that in some cases of pelvic inflammation there is an inflammatory condition of the peritoneum itself."

Scanzoni makes no allusion whatever to inflammation of the pelvic cellular tissue, and devotes but a single page to the description of peri-metritis, which he considers to be most frequently a puerperal disease, but occasionally a secondary affection "accompanying an acute or chronic inflammation of the uterus or the ovaries, or one of the various neoplasmata which are developed in these organs."

His account of it is so very meagre as to be of very little practical value. Nonat, who may in some measure be regarded as the French representative of the views most prevalent among English gynæcologists, locates the inflammation chiefly in the cellular tissue, describing it under the name peri-uterine phlegmon, of which he makes four varieties, viz: "the lateral phlegmon," "the phlegmon of the broad ligaments," "the retro-uterine phlegmon," and "the ante-uterine phlegmon." The lateral phlegmons differ from those of the broad ligaments (which of necessity are also lateral) in being in such close contact with the uterus as to seem to make a part of it. Previously to the researches of Nonat, this close proximity to the womb had caused them to be regarded as due to partial parenchymatous metritis. Although Nonat does not deny the occasional occurrence of pelvic-peritonitis either as a complication of peri-uterine phlegmon, or independently of it, he gives no account of it as a distinct affection, and indeed barely alludes to it. Bernutz, on the contrary, takes a position directly opposite to that of Nonat, and after proving to his own satisfaction that peri-uterine phlegmon is anatomically impossible, he proceeds to show that the signs and symptoms supposed to characterize that condition may all be caused by pelvic-peritonitis, without any implication whatever of the cellular tissue; and in his opinion they always *are* so caused. He sustains his position by the evidence of several autopsies. I give an abstract of his views in his own words:—

"The slightest dissection shows that the cellular tissue subjacent to the peritoneum is so thin and scanty that it is impossible to separate the serous from

the uterine tissue; and that consequently it cannot be the seat of swellings, which, according to M. Nonat's observations, attain in the space of a few hours to the size of a hen's egg. * * * It is, therefore, anatomically impossible that the tumours which M. Nonat called uterine-latero-phlegmons, and which are quite distinct from phlegmons of the broad ligaments, can be situate in the cellular tissue of the lateral parts of the womb. * * * From all these facts I have concluded that cases of supposed peri-uterine phlegmons ought to be classed with those of partial peritonitis."

Thus we find Bernutz claiming for peritonitis precisely what Nonat claims for cellulitis, and although he does not deny the existence of the cellular inflammations, he gives no account of them and treats only of pelvic peritonitis. Aran adopts the term peri-uterine inflammation, and says of it: "I propose under this name to describe an affection that has by turns received the denomination of inflammation of the lower belly, inflammation of the appendages of the uterus and of the broad ligaments, inflammation of the pelvic cellular tissue, and pelvic peritonitis." Courty also adopts the term peri-uterine inflammation, which he says is preferable to any other name precisely because it comprehends all the others and prejudges nothing as to the seat of the malady. Thus far, then, we find the English writers regarding these diseases as essentially inflammatory affections of the cellular tissue, the peritoneum being occasionally involved. In France we have Nonat treating of cellulitis as the principal affection, Bernutz controverting this theory and substituting peritonitis for cellulitis, while Aran, Courty, and others resort to the term peri-uterine inflammation expressly to avoid committing themselves to either theory. It does not appear to have occurred to any one prior to the year 1868 to dissociate these diseases entirely and to assign to each its proper position as an independent affection, having no more necessary connection with the other than has pleurisy with pneumonia. Bernutz, it is true, in treating of pelvic peritonitis differentiates it from phlegmons of the iliac fossa, in which he erroneously includes cellulitis of the broad ligaments, but he is so intent on convicting Nonat of error, that he barely recognizes cellulitis and gives it no further consideration. Virchow, also, makes a pathological distinction between these two affections, giving the name perimetritis to the peritoneal inflammation, and para-metritis to that of the cellular tissue; but no clinical separation of them is to be found in any systematic treatise on diseases of women, that I am aware of, earlier than the date I have mentioned. The first distinct enunciation of the fact that they are entirely separate affections, and can generally be distinguished from each other clinically, I find in Dr. Thomas's treatise on Diseases of Women first published in the beginning of the year 1868. The two diseases are therein described separately, each with its own history, etiology, symptoms, and diagnostic features. This distinction once made, it becomes a matter of surprise that it should not have been made sooner. That partial or local peritonitis is of common occurrence has long been known—that inflammation and abscess of the cellular tissue are also frequent affec-

tions has been equally well known; that they should have been so long confounded in this locality has probably been due to the incompleteness of our observations, the inadequacy of our means of diagnosis, and the infrequency of opportunities to confirm or correct a diagnosis by post-mortem examination, these diseases rarely proving fatal. Nearly a year after the appearance of Dr. Thomas's book, Dr. Matthews Duncan published his monograph on peri-metritis and para-metritis, which is by far the most complete exposition of what is known of these diseases that has yet been put forth. He not only recognizes cellulitis and peritonitis as independent affections, but subdivides each into varieties of which he treats at considerable length. "These affections," he says, "are surpassed in importance by none of the diseases of women; they are common, painful, and dangerous both to functional perfection of the parts implicated and to the life of the sufferer from any of them." By peri-metritis, he means inflammation of the uterine peritoneum; by para-metritis, inflammation of the cellular tissue in connection with the uterus. Of this nomenclature, which he takes from Virchow, I would observe in passing, that, while the term peri-metritis is unobjectionable, its meaning being obvious enough, and sufficiently precise, the term para-metritis requires to be explained, and is, therefore, inferior to peri-uterine cellulitis, which explains itself, and which he rejects because of its hybrid etymology. The substitution of the word peri-metric for peri-uterine might avoid offence to the etymologist and enable us to retain a name that indicates the seat and nature of the malady. Coming now to the clinical consideration of these diseases, and accepting the fact which is everywhere admitted that the inflammation sometimes attacks the peritoneum, and at others the cellular tissue, almost the first question that confronts us is this: Have we any means of determining in a given case which of these tissues is the seat of the disease?

Dr. Thomas says of peritonitis and cellulitis, "that they may usually be readily differentiated from each other, and a neglect of the effort at such thorough diagnosis is as reprehensible as a similar want of care in determining between pericarditis and endocarditis." Matthews Duncan thinks otherwise. He says of the diagnosis of these affections from each other, that "it is still in a very unsatisfactory state." That, "while the very existence of the various affections is still, in some quarters at least, regarded as an open question, there must surely be at least difficulty of diagnosis to justify doubt." He quotes the tabular statement in which Dr. Thomas contrasts the distinguishing characteristics of the two diseases, merely to express his dissent from it as a whole, and in its several parts, and then proceeds to say:—

"I shall in the mean time only say that we are indebted to Thomas for even attempting the difficult task, and that I shall not attempt to supplant his diagnosis by one of my own. I have often felt satisfied, from careful observation of a case, at many stages of its progress, that I could make a good guess as to the peri-metric or para-metric nature of an abscess. The indications that Thomas

lays down show the kind of indications that I have relied upon. I am not, however, prepared to state them categorically."

From this it would appear, that he regards the cases in which a differential diagnosis can be made as exceptional, but that the indications given by Dr. Thomas are, in the main, correct. For my own part, while I am less confident than Dr. Thomas, I am by no means so doubtful as Matthews Duncan. In about two-thirds of all the cases that have come under my observation, I have thought the diagnosis clear enough to be susceptible of demonstration; the element of doubt, however, has entered very largely into the other third. I should state that most of my cases have been seen among out-door patients coming to the Demilt Dispensary, the Woman's Hospital, and the Clinic for Diseases of Women at the College of Physicians and Surgeons. They were, therefore, nearly all subacute or chronic cases. The acute cases that I have seen have been comparatively few in number, and have occurred mostly in private practice.

Entertaining the belief that subacute and chronic cases will always form the majority of these affections in general practice, I shall invert the usual order of sequence and speak of them first, beginning with the physical signs on which we must chiefly depend for diagnosis, there being nothing in the symptoms sufficiently distinctive.

The uterus, as is well known, possesses a considerable degree of mobility. It may be drawn down to the vaginal outlet, it may be pushed high up in the pelvis, and it may be forced to either side. In women who are not too fat, or who have not an unusual rigidity of the abdominal muscles, that is to say, in the majority of women, the uterus may be distinctly felt through the abdominal walls by making pressure above the pubes with one hand, while counter-pressure is made in the vagina with the index finger of the other hand. It may be thus included between the two hands, and its size, shape, position, and degree of mobility determined with great accuracy.

Signs and Symptoms of Subacute Cases.—Now, one of the first and the most constant of the effects of pelvic peritonitis in any of its severer forms, is to impair or abolish this mobility. The finger in the vagina impinges on the uterus, but can neither push it up, nor draw it down, nor move it to either side. There is fixation, more or less complete. Not only is there immobility of the uterus, but there is hardness and rigidity of the whole vaginal roof. Whether the finger be carried behind the cervix, or in front, or on either side of it, it meets with a firm resistance of almost cartilaginous density. Occasionally this hardness may not entirely surround the cervix, but in severe cases it nearly always does. In the milder attacks the induration of the pelvic roof may be slight in degree, limited in extent, or entirely absent, but there is always some fixation of the uterus. There is, moreover, almost invariably, some displacement of this organ. It may be flexed backwards or forwards, it may be anteverted or retroverted, or drawn to either side, but a deviation of some kind almost always exists;

and the uterus is held in its abnormal position by adhesions. There is, too, in a comparatively small number of cases, a distinct tumour to be felt, located most commonly behind the cervix, in the posterior cul-de-sac, but occasionally on one side and in close proximity to the uterus. This tumour can rarely be felt through the abdominal walls, and is usually to be detected only in the vagina. These constitute all the physical signs of pelvic peritonitis, as it is seen in subacute and chronic cases, and they may be briefly summed up as follows: Displacement and fixation of the uterus, hardness of the vaginal roof, and the occasional existence of a tumour in such close proximity to the uterus as to appear to be a part of it.

Let us now consider the physical signs of cellulitis of a subacute or chronic character. In examining a case of this kind the most prominent abnormality that we find, and that which is most likely first to arrest attention, is the existence of a tumour, usually on one side of the uterus and in about the situation of the broad ligament. This tumour, in many cases, feels to the touch not unlike the body of the uterus drawn to one side and fixed there; for it is often no larger in size and not dissimilar in shape. Much less frequently the tumour may be felt posteriorly, and in some instances a tumour may be found on each side of the womb. These cases, however, are exceptional. The tumour is nearly always immovable, and can almost invariably be felt through the abdominal walls when firm pressure is made. The mobility of the uterus is generally diminished, but there is no such fixation as occurs in peritonitis unless that disease complicates the cellulitis. In a considerable number of cases there is no impairment whatever of its mobility. There is usually no induration of the pelvic roof unless in the immediate vicinity of the exudation, and commonly not even there. As a rule, the uterus suffers little or no displacement, and when displaced is not fixed in its abnormal position. These are all the physical signs of cellulitis: let us contrast them with those of peritonitis.

In peritonitis we have displacement and fixation of the uterus, diffused hardness of the pelvic roof, and commonly no tumour. In cellulitis we have always a tumour, generally on one side of the uterus, little or no hardness of the pelvic roof, and not much displacement or fixation. There would appear to be sufficient dissimilarity here on which to base a differential diagnosis, and in typical cases I think such a diagnosis can always be made; but, unfortunately, all cases are not typical. Peritonitis is a very frequent complication of cellulitis, and occasionally cellulitis is sufficiently diffuse to resemble very closely peritonitis. The question might here be asked: what evidence is there that the physical signs just given are distinctive of these affections even in typical cases? To such a question I would answer that the evidence is of two kinds, direct and inferential.

Evidence of the first kind is afforded by autopsies of subjects dying while suffering from these affections in whom the lesions discovered after death

are such as correspond with the signs that I have mentioned. For example, the most carefully observed cases of this kind that I have been able to find are those of Bernutz, which were all cases of peritonitis, and were published by him for the express purpose of showing how constantly this affection has been mistaken for cellulitis, and how very grievously Nonat had erred in this respect: consequently they were cases in which the question of differential diagnosis received the most careful attention. Now the two most prominent physical signs of pelvic peritonitis that I have mentioned are displacement and fixation of the uterus, and these were found in every one of Bernutz's autopsies. He says but little of hardness of the pelvic roof, and the presence of a tumour is the rule in his cases, and not, as I have stated it, the exception. But as all of these cases were of that very exceptional variety of pelvic peritonitis in which suppuration takes place, and forms an encysted tumour, they constitute just that class of rare cases that I have referred to as sometimes giving a tumour, and in which hardness of the pelvic roof would not be likely to be a prominent feature. In fact, Bernutz himself calls attention to the great rarity of suppuration in pelvic peritonitis.

The second kind of evidence is in reality nothing more than a corollary of the first; for if in examining any given case a tumour be found in the vicinity of the uterus without displacement and fixation of that organ, then we may infer that that tumour is not due to peritonitis. Moreover, both clinical and necropsic observations have shown that the adhesions of peritonitis are very persistent unless removed by pregnancy, while the tumours due to cellulitis are more commonly of only temporary duration, often disappearing entirely in a few weeks.

Let us now see how far the symptoms may aid us when the physical signs leave us in doubt. I have already intimated that these are almost never sufficient of themselves, but they may occasionally afford us some assistance. I would remark, then, in the first place, that, in a very considerable number of subacute and chronic cases, perhaps in one-half, the symptoms are so slight, or have so little reference to any inflammatory condition, that they do not point to these affections at all. It is a very common circumstance to be consulted by patients on account of dysmenorrhœa, or irritability of the bladder, or suppression of the menses, or metrorrhagia, and find on examination evidences of extensive cellulitis or peritonitis of which no history can be given by the patients. They cannot remember to have suffered from any illness to which they could be ascribed.

In other cases, however, the history and symptoms are sufficiently well marked. The one symptom that is most prominent in both affections is *pain* in or about the pelvis. This pain is sometimes referred to one or the other iliac fossa, sometimes to the supra-pubic region, and sometimes to the back. Occasionally it shoots down the thigh, and may even cause retraction of the limb. There is nothing in this symptom on which we

can predicate with any certainty which affection we are dealing with. It may be stated, however, that pain confined to one side or shooting down one thigh, more frequently attends cellulitis, while pain of a paroxysmal character is oftener peritoneal. Retraction of the thigh is almost always the result of cellulitis. In the functional disturbances of the ovaries, uterus, bladder, and rectum there is nothing that is distinctive of either affection.

Frequency of Suppuration.—Suppuration, when it exists, is in most cases due to cellulitis. I say *when it exists*, for in my experience it is of far less frequent occurrence than we would be led to suppose from the statements of most authors on this point. For example; West says: "The tendency to suppuration and to the discharge of pus seems to be very great, since it occurred in 27 out of 52 instances. This mode of termination of the inflammation appears, also, to be as frequent in cases independent of previous delivery or miscarriage, as in those which are due to puerperal causes, since it happened in 9 out of 15 intances of the former kind, as well as in 18 out of 37 of the latter." McClintock states: "As regards the frequency of abscess in puerperal cellulitis, my clinical records show the following results. Of 70 cases, 37 ended in suppuration, with discharge of pus, and 24 of these burst, or were opened externally." He gives no estimate of its comparative frequency in non-puerperal subjects, although one of his two chapters is devoted exclusively to this class of cases. Simpson was of opinion that suppuration occurs in about one-half of all the cases. Bennett regards suppuration as the more frequent termination, as did also Grisolle, and some others of the French writers. Duncan gives it as his opinion, that suppuration in these cases is even more frequent than the statements of any of these authors would lead us to suppose. Dr. Thomas, on the contrary, says: "Cellulitis proper, that is, uncomplicated by other diseases, rarely passes into a chronic state, but usually, in the course of two or three weeks, passes off by resolution, or ends in suppuration, the former being much the more frequent termination;" and yet, in his tabular synopsis he gives a tendency to suppuration as one of the distinctive characteristics of cellulitis. Nonat states that suppuration, which has been regarded as a very common termination by most physicians, is in reality very rare, when the cellulitis has been subjected to proper treatment. Gallard met with it only 4 times in 53 non-puerperal cases. Aran's experience agrees with that of Gallard, and Courty is of the same opinion as Aran.

I have dwelt somewhat upon this question of suppuration, because I consider it an important one, and because this discrepancy in opinion and statement seems to me to be very significant. This is a question, not of theory, but of fact; how then are we to explain such contradictory statements on the part of capable and conscientious observers? I think the solution is to be found in the circumstance to which allusion has already

been made, that a large proportion of cases are attended with so few or such slight symptoms at the outset, that they escape notice, and are only detected, if detected at all, when some subsequent ailment or functional disturbance causes investigation; while, on the contrary, every case of sufficient severity to result in suppuration must almost inevitably challenge attention.

Nearly every author to whom I have referred bases his opinions or statistics on cases observed in hospital practice. Now, among puerperal patients, all suppurative cases would almost certainly be reckoned, while the milder cases might easily escape detection; and the same result would obtain among non-puerperal patients. Among out-door patients, however, the case would be different. They almost always seek advice on account of some local pain or functional derangement that necessitates a careful examination, and among these, evidences of cellulitis and peritonitis could scarcely escape observation, and, indeed, are commonly found where the history given by the patient would never have led us to suspect their existence. I am aware of a possible source of error here, in the fact that suppuration may occur, and the pus be discharged through the rectum or bladder without the patient's knowledge, but after making all due allowance for cases of this kind, suppuration must still be regarded as quite an exceptional occurrence. Among my own cases, it occurred 3 times in 49 cases of cellulitis, and once in 16 cases of peritonitis. I believe I have now mentioned all the symptoms that are of any value, and I think it is obvious enough that they are insufficient of themselves for a diagnosis.

Signs and Symptoms of Acute Cases.—Leaving now the consideration of subacute and chronic cases, let us see if we can do any better, or as well, with acute cases. Speaking from a comparatively small experience, extending to only nine recorded cases of cellulitis and seven of peritonitis, I should answer, *not so well*, at least in the early stages, by which I mean the first four or five days of the disease. In cases of this kind, the symptoms become rather more valuable, and the physical signs rather less so. With the symptoms of peritonitis we are all sufficiently familiar, and pelvic peritonitis differs from general peritonitis only in being less severe and more localized; that is to say, we have the same pain, fever, nausea and vomiting, dorsal decubitus, anxiety of countenance, etc., but the tenderness and pain are chiefly in the hypogastrium and pelvis; tympanites is either entirely absent, or slight in degree, and there is more commonly functional disturbance of the pelvic viscera. Now, in cellulitis we have also pain and tenderness in the hypogastrium, fever, sometimes, but less commonly gastric disturbance and interference with the functions of the bladder and rectum. There is certainly no great difference here, and yet in a considerable number of cases there is such a difference in the physiognomy of the two affections as would enable an experienced observer to

form a pretty correct judgment from the symptoms alone. The physical signs are somewhat better marked, but are by no means so distinctive as in subacute and chronic cases. In peritonitis, physical examination usually reveals nothing during the first five or six days, but tenderness when the uterus or pelvic roof is touched, and pain on hypogastric pressure. After this, there begins to be a general hardness of the pelvic roof, with fixation of the uterus, and if there be any distinct tumour, it will be found most frequently in the posterior cul-de-sac. In cellulitis, on the other hand, besides pain and tenderness when the uterus and roof of the pelvis are touched, there is also, very early, a feeling of fulness which is commonly more pronounced on one or the other side of the uterus; not a well-defined tumefaction, such as occurs later, but an œdematous sort of swelling which may resolve into a distinct tumour in two or three days, or only after the lapse of a week. These differences, slight as they are, are the only ones, I think, that clinical experience has thus far enabled us to point out.

Another question might here be raised, and in fact *has* been raised, as to whether there is any practical advantage to be derived from the differentiation of these affections, even assuming that this can generally be accomplished. I think it can be shown that there *are* palpable advantages, but I shall not now rehearse them, for, in my judgment, a question of this kind should never be entertained for a moment. It is the duty of those engaged in scientific investigations, to take note of every fact, or supposed fact, that comes under observation, and to give the results of their inductions, without any reference to the question of immediate practical utility. It is just about a century ago, that measles and scarlet fever began to be recognized as distinct diseases, and we have only to go back to the time of Sydenham to find a similar distinction beginning to be made between smallpox and measles. It is no more likely that there was any immediate practical utility apparent in the first separation of those diseases than there now is in the differentiation of the affections under consideration, and there was certainly in those days, as in the present, no lack of opposition to these over-refinements, so-called, of diagnosis. The possible utility of clinical and pathological distinctions of this character, is a matter which only the future can fully determine, and it is unphilosophical and idle to attempt to estimate it in advance.

Frequency of Cellulitis and Peritonitis.—I now propose to speak very briefly of the frequency of these affections, of their causes, their diagnosis from other affections, and their treatment. Of their frequency, as compared with that of other uterine diseases, only an approximate estimate can be made. Courty, grouping both diseases together, considers that, either as primary affections or as complications of other maladies, they constitute about one-third of all uterine ailments. Aran found evidences of pelvic peritonitis in twenty-nine of fifty-three autopsies of females dying in his service. But, as Matthews Duncan justly observes, these were

practically selected cases. They were not fifty-three women taken at random, but fifty-three women taken from a gynæcological service, and suffering from ailments so severe as to cause death. Simpson, West, and Duncan merely speak of them as being of frequent occurrence.

From my own experience I should say, that while these affections are far more common than has generally been supposed, they are by no means so frequent as Courty estimates. Of more than four thousand recorded cases of uterine disease of all kinds that I have seen during the past seven years in the different institutions to which I have alluded, and in private practice, I find only 109 entered on the books as due to cellulitis or peritonitis. This, however, does not represent the actual number of these affections that came under observation, for in nearly all the cases of flexion or version of the uterus in which adhesions were found, the displacement alone is entered as the diagnosis, no note being made of the peritoneal complication. Many cases, too, were undoubtedly overlooked, more especially during the first two or three years of this period; but after making a liberal allowance for these omissions, I am sure that the relative proportion of cases of this kind that can be diagnosticated does not exceed one in ten.

Causes.—The causes of these affections I shall merely enumerate, dwelling for a moment on one only. They are chiefly these:—

Parturition or abortion.
Endo-metritis or ovaritis.
Operations or injuries.
Escape of fluids into the peritoneum.
Exposure to cold.
Gonorrhœa.

It is the opinion of Matthews Duncan, that of all these causes, endo-metritis is almost the only one to be taken into account. He says:—

"The theory on which I insist is, that these inflammations are all secondary, that they are produced by inflammation of the uterus, or of the tubes, or of the ovaries, or by noxious discharges through or from the tubes and the ovaries, or by mechanical injury. * * * Of all of the prolific causes, inflammation of the mucous membrane of the womb is, in my opinion, the most common, and this both in the puerperal and non-puerperal states." Again he states, "Now I assert, that in the majority of cases, pelvic abscess and metritis are stages and conditions of one disease."

With regard to this theory I would observe, that while endo-metritis is not at all an infrequent cause of these affections, I do not believe that they are by any means so generally dependent upon it as Duncan states. It seems to me, that local peritonitis may be just as much an idiopathic affection as local pleuritis, and I see no reason why the starting-point of the inflammation in these cases should be the uterus any more than the peritoneum or the ovary. I have certainly seen many cases of cellulitis and peritonitis that could not be traced to any concomitant or antecedent

endo-metritis. There is, moreover, a certain pathological law bearing upon the propagation of disease by continuity of tissue (which is the method implied by Duncan's theory) that is worthy of consideration in this connection. It is thus stated in Dr. Peaslee's work on human histology. Speaking of the varieties of epithelium and their distribution, Dr. Peaslee says:—

"Disease respects the distinctions made in regard to the different varieties of epithelium. In croup the nasal passages are almost invariably first affected and the disease follows the course of the ciliated epithelium over the posterior surface of the velum, and thence into the larynx and trachea, and *not* along the œsophagus into the stomach. Again, a disease commencing in the lower half of the pharynx, or the tonsils, does not soon extend to the larynx and trachea, as a general rule. Besides, the uterine glands may be diseased for an indefinite period, without the disease extending either to the uterine cavity or to the vagina; its conoidal epithelium being bounded both above and below by the scaly variety."

Although this is a question of great interest, the limits of this paper will not allow me to pursue it further.

With regard to the influence of the puerperal condition in giving rise to these affections, I find one statement in the work of Dr. Thomas that is not sustained by my own experience. He lays it down as a proposition that "peri-uterine cellulitis is very rare in the non-pregnant woman, while pelvic peritonitis is exceedingly common." I think both members of this proposition need confirmation, and especially the first. My own records show that out of 49 cases of cellulitis, 11 followed labour or abortion; 5 were consequent upon operations, and in the remaining 33 the cause was either not known or not ascertained. Of 16 cases of peritonitis 5 were due to labour or abortion, 4 to operations, and the other 7 to causes not known or not stated. Ten of the cases of cellulitis and six of peritonitis occurred in nulliparous women.

Having pointed out some of the means by which these affections are to be distinguished from each other, we have now to consider with what other diseases they are likely to be confounded, and how a differential diagnosis may be made; and here I shall have to be very brief, for although this is a most important part of the subject, it is too extensive to be treated here at any length. The principal diseases from which they are to be differentiated are:—

Hæmatocele.

Fibrous tumours.

Uterine deviations.

Displacement and fixation of the ovary.

Fecal infarction.

Hæmatocele differs from both cellulitis and peritonitis in being much more rare than either, in occurring with suddenness, and most frequently during a menstrual period, in being accompanied with hemorrhage, in giving the constitutional symptoms of loss of blood, and in occasioning a

tumour that is at first soft and elastic, and only becomes hard when peritonitis complicates it or when the fluid portions of the blood have been absorbed. The most efficient of all means of diagnosis is, however, in the majority of cases the use of the hypodermic syringe, or the fine trocar that accompanies the aspirator of Dieulafoy. Fibrous tumours are distinguished by their mobility, the absence of pain or tenderness on pressure, by their evident attachment to the uterus, and by the absence of any signs of inflammation. The free mobility of a prolapsed ovary, whether enlarged or not, would serve to distinguish it from the tumour of cellulitis with which alone it would be likely to be confounded. Fixation of the ovary would be an evidence of peritonitis. Uterine displacements are best determined by the use of the uterine probe or sound. Fecal impaction high up in the rectum is likely to prove a source of error only so long as it is unsuspected. Nevertheless I have known several errors of diagnosis on this account, and the real disorder to be aggravated by the use of opiates given to relieve the symptoms of a cellulitis that had no existence. The pain, and tenderness on pressure, that commonly attend these affections, often render it impracticable to make such thorough exploration as is necessary for a satisfactory diagnosis. Under these circumstances anæsthetics are of inestimable value. I have frequently succeeded in making a diagnosis with their assistance, that would otherwise have been impossible. I think their full value for this purpose is not yet generally appreciated.

On the subject of *prognosis* I must be still more brief. So far as danger to life is concerned, there is, in most cases, nothing to be apprehended. Functional derangements of the ovaries, uterus, bladder, and rectum persist for a variable period, which is generally much less in cellulitis than in peritonitis. All evidences of cellulitis may pass away in a few weeks, but the signs of peritonitis, unless removed by pregnancy, generally remain for years. The subject of treatment I shall also dismiss in a very few words, and those few will refer chiefly to subacute and chronic cases of the non-suppurative variety. I will merely observe that the acute stage of either of these affections should be treated on general principles—anodynes, local depletion, fomentations, and sedatives are the chief therapeutic agents. After this stage has passed, the most efficient means of causing the absorption of the products of inflammation are, for both affections, successive fly-blisters over the hypogastrium, and the copious use of the warm vaginal douche every night and morning. The best apparatus for this is a keg that will hold two or three gallons, having an India-rubber tube attached to the stopcock. This keg being filled with water, as warm as the patient can bear, is placed upon a shelf ten or twelve feet high; the patient being suitably arranged on a lounge or chair underneath, the free end of the tube is introduced high up in the vagina, and the water allowed to flow in a constant stream against the pelvic roof and the neck of the

uterus. The rapid improvement that follows the use of these means in the majority of cases is astonishing even to those who have had much experience in the use of them.

Having now considered as fully as the occasion will permit all the points to which I purpose calling attention in this paper, it only remains that I should exemplify some of them by illustrative cases. Before doing this I would state that the whole number of cases of which I have kept a record, is 87. Of these, however, 22 are so incomplete as to be of very little value, and I have cast them out. The remaining 65 give the following results:—

	Cellulitis.	Peritonitis.
Whole number of cases	49	16
Acute cases	9	7
In married women	44	13
In single women	5	3
In nulliparous women	10	6
Tumour on right side	17	1
Tumour on left side	28	1
Tumour on both sides	4	14
Following labour or abortion	11	5
Following operations	5	4
Cause not ascertained	33	7
Resulting in suppuration	3	1

Case 1. *Pelvic Peritonitis.*—March 29, 1868, I was requested to see Mrs. S., æt. 32, mother of seven children. On the 20th of the preceding December (that is to say, rather more than three months before I saw her) she had aborted at about the middle of the third month of pregnancy. She knew of no cause for the abortion, and had never suffered from any symptoms of a uterine disease. She kept her bed for eight days, but on the second day after getting up, she was taken suddenly with severe pain in the hypogastrium, attended with faintness, nausea, and vomiting. Acute febrile symptoms then came on, and she was very ill for five days. There was great tenderness over the whole of the hypogastrium, distressing nausea and vomiting, and constipation of the bowels. About the sixth day of the attack the fever began to abate and the pain to wear away, but up to the time of my visit, three months afterwards, she had never recovered sufficiently to attend to her household duties, and had never been out of bed more than three consecutive days. Her disability was occasioned by weakness, hypogastric pain, and occasional febrile attacks with chills, that would occur every five or six days, the interval varying from five to eight days. She had menstruated twice, losing on both occasions unusual quantities of blood, and suffering from dysmenorrhœa, to which she had never before been subject. There had also been pretty constant vesical tenesmus and dysuria. Her medical attendant had evidently failed to appreciate her condition, for although he had been diligent in the use of many drugs, it had never occurred to him to make a vaginal examination, or in any way to interrogate the pelvic viscera, and I could not help suspecting that inappropriate treatment had done quite as much to exhaust her strength as the disease itself. On making a vaginal examination, I found the uterus anteverted and completely fixed, and the pelvic roof everywhere hard and

unyielding. There seemed to be more tenderness on the left side, and on making pressure over the left iliac fossa, I thought I could detect a greater fulness on that side, but no distinct tumour could be made out. I regarded the case as one of pelvic peritonitis following abortion, and as it was the first case of the kind I had seen since my attention had been drawn to the differential diagnosis of cellulitis and peritonitis by Dr. Thomas, I was much interested in it. While explaining to the patient what I thought to be the nature of the disease, and what could be done for her, I incautiously hazarded the opinion, that she would never have any more children, the displacement and fixation of the uterus being such as to render conception very improbable, and abortion almost inevitable if pregnancy *should* occur. As she had already given birth to seven children, I found that this probable arrest of fecundity had no terrors for her, and was rather a source of satisfaction than otherwise. The treatment consisted of successive blisters over the hypogastrium, the free use of the warm vaginal douche every night and morning, and such tonic treatment as her enfeebled condition suggested. She began to improve at once, and in the course of a month I ceased to see her. On the 15th of the following November I was called to attend her in an abortion occurring at the beginning of the third month of pregnancy. I have since delivered her of two living children, one in December, 1869, and the last in January of the present year.

Both the symptoms and the physical signs in this case pointed unmistakably to peritonitis; and the manner in which the adhesions were gradually overcome and removed by pregnancy was highly instructive.

Not long after this I saw a very similar case in the practice of Dr. J. L. Campbell, of this city, but one of much greater severity, and following labour at full term. The doctor had promptly recognized the nature of the affection, and had asked me about three months after the attack to take the patient under my care. The same treatment was pursued, the same prognosis as to future pregnancies was given, but with much greater emphasis, and the same result attained. The woman became pregnant in less than a year, passed through the period of gestation without aborting, and gave birth to a living child. It is no exaggeration to state, that in this case, every part of the pelvic roof was, as Docharty expresses it, "as hard as a deal board."

Case 2. *Peri-uterine Cellulitis.*—About the middle of last August, I was urgently requested to go to a watering-place some two hundred miles distant, to see a lady who had been taken suddenly ill with some pelvic inflammation, and was thought to be in great peril. On arriving at the hotel, the husband of the lady, a gentleman about forty years of age, gave me the following particulars of the case. His wife, a lady about thirty-two years of age, having already given birth to five children, was anxious to avoid another pregnancy. As he had come to this place with his family to spend a couple of months in complete relaxation, and had nothing special to occupy his mind or divert his thoughts, his sexual impulses became very importunate, and he placed no restraint upon them, not even respecting the menstrual period. Having heard somewhere that lemon-juice was an excellent preventive to conception, it occurred to him to make a trial of it in a concentrated form, as a substitute for some of the other

precautions that he had been accustomed to employ. Accordingly he made a pretty strong solution of citric acid, which was injected into the vagina directly after a sexual act, occurring during a menstrual period. The woman was almost immediately taken with severe pain in the pelvis, followed by high fever, and other symptoms of acute inflammation. The resident physician of the place was called in, who, not feeling equal to the emergency, sought the aid of a physician of this city, who chanced to be stopping at the hotel for a few days. This gentleman saw the patient on the third day, I think, of the attack. On making an examination, he found the uterus exceedingly tender to the touch, and a fulness about the vagina, which, in view of the fact that the attack had come on suddenly during a menstrual period, he very naturally attributed to the presence of an hæmatocele. The patient was placed under such treatment as her immediate symptoms required, but as she continued to be very ill for several days, and the consulting physician was obliged to return to New York, the husband became alarmed, and sent here for further advice. I saw the lady about the ninth or tenth day of the disease. The acute symptoms had then in a great measure subsided, and I found her suffering mostly from exhaustion, due to vomiting and want of sleep, the opium that she was still taking having the effect of making her morbidly vigilant, and keeping her profoundly nauseated. On examination, the uterus was found still tender, but neither fixed nor displaced. Its mobility was somewhat interfered with by a firm, immovable tumour in the right iliac fossa, about as large as an orange of medium size. There was no swelling directly behind the uterus, or on the left side, and no hardness of the pelvic roof. There was a muco-purulent discharge from the os uteri and vagina, which indicated clearly enough, the starting-point of the inflammation. The case was obviously one of endo-metritis and cellulitis, without any peritoneal complication. I discontinued the opium, and gave the patient thirty grains of chloral by the rectum, resorting to this method on account of her nausea. She slept soundly that night, and began to convalesce at once. The warm douche and blisters were recommended, and when I saw the lady about a month afterwards in this city, the tumour had decreased in size fully one-half. As her home is some distance from here, I have not since seen her.

Case 3. *Cellulitis.*—E. W., æt. 29, widow, mother of three children, the youngest being 3½ years old, came to the Demilt Dispensary, 6th of last January, on account of a uterine hemorrhage of five weeks' duration. She stated that she had been taken with pain in the hypogastrium about five weeks ago without any apparent cause, the attack occurring about a week after the menstrual period. It was accompanied with a chill, and some fever. On the third day of the attack she began to flow, and had continued to flow ever since. Was confined to bed only four days. On examination, nothing abnormal was found about the uterus or vagina; but a tumour, about as large as a turkey's egg, was discovered in the situation of the right broad ligament. It was tender to the touch, and immovable, and could readily be felt through the abdominal walls. The same treatment was pursued here as in the other cases, and when I last saw the patient about a week ago, the tumour was no larger than a hickory-nut. The hemorrhage ceased almost as soon as the treatment was commenced.

This patient, it will be observed, sought advice solely on account of hemorrhage, and I have seen perhaps half a dozen similar cases. I have

now under treatment at the Woman's Hospital, a lady suffering from cellulitis, who was admitted chiefly on account of metrorrhagia from that cause.

I had intended to introduce here the histories of some cases tending to illustrate the difficulties of diagnosis that are sometimes encountered, and of which the service in the Strangers' Hospital has furnished me with several very interesting examples, but this paper has already grown to such a length that I am compelled to omit them.

In conclusion, I would observe that no one can be more fully aware than myself, of the very imperfect manner in which this subject has been presented, but if the remarks here made shall serve to awaken sufficient interest in these affections to induce others of larger experience and more mature views to contribute something, from the stores of their knowledge, to the settlement of the many vexed questions that hang around them, my object will have been fully attained.

ART. III.—*On the supposed Oxytocic Action of Cinchona.*

By JAMES BORDLEY, M.D., of Centreville, Md.

THERE seems to be a growing conviction in the minds of many that cinchona exerts a direct influence over uterine contractions. But after extended experience with the drug, I am forced to the belief that it has neither direct nor indirect physiological influence over the organ. Living, as I do, and practising, in a region greatly infested by malaria, and therefore compelled to use the bark and its preparations daily, I feel fully prepared to answer the question in the negative. Had it been an agent of as powerful emmenagogue properties as some have asserted, it could not have failed to have produced those results in cases under my care. I have in remembrance many cases in which the sulphate of quinia was administered daily in large and often-repeated doses (as high as ten and fifteen grains each) during all the varied stages of gestation, without a single case in which such a result followed its use. One case I call to mind which more particularly illustrates the point.

Mrs. S. in sixth month of her pregnancy was attacked with most obstinate form of remittent fever. I was called to see her, and ordered thirty grains of quin. sulph. in six doses, one to be given every three hours: after and even before the administration of the last dose she exhibited all the physiological effects of the medicine (I might here say that she was abnormally susceptible to its action, and showed in a marked degree all its constitutional effects); upon visiting her the next day and finding that there had been a second remission, I repeated the prescription, but with like

result, and this treatment was kept up, with some slight variation, for eight or ten days, during which time I never allowed the action of the drug to cease; lessening the quantity as the disease gave way to its influence. I have since that time delivered her at full term of a healthy male child.

I am aware of the folly of attempting to establish any principle upon the results of a single case, and such is not the fact in this instance. I simply cite this one as bearing more directly upon the question in point than any I can now call to mind, having kept no record of my cases, not having treated them with experimental ideas. But, as I said before, I am daily treating such cases, and I have yet to see the first case in which such results have been produced by its exhibition.

I cannot agree with Dr. Monteverdi, when he states that the emmenagogue properties of the bark are superior to ergot, and if carried far enough will produce expulsive contractions which will at last result in miscarriage and premature birth, as the result of its physiological action. Might we not look for the same result from the administration of any drug carried to its toxical effects?

When we consider the highly impressible state of the nervous and circulatory systems present in pregnancy, we are not surprised to see disastrous consequences following the exhibition of any substance which carries its excitation beyond the limits of prudence, or lowers the vital powers in a morbid degree: and my experience with the article now under consideration leads me most conclusively to assert that it must be given in dangerous quantities to effect Dr. Monteverdi's result; and I do not allude simply to the danger due entirely to miscarriage, but to its general constitutional effects; especially those acting upon the nervous system. It appears to me, that, in the highly excitable condition of the nervous centres existing during the latter months of pregnancy, the peculiar action of quinia carried too far would render the patient liable to any or all of the disasters peculiar to the parturient.

My opinion then based upon my experience leads me to go so far as to affirm as my decided conviction, that the true physiological action of cinchona, so far from producing any uterine contractions, tends directly to control such an action and to suspend contractions which threaten abortion. I allude more particularly to those threatenings which are the result of malarial disturbance. If it be true, as is asserted by some, that cinchona diminishes the proportion of fibrin in the blood and lessens its coagulability, it seems to me a most dangerous remedy to employ for the purpose, even were we to admit that it possessed such emmenagogue power when given in rational quantities.

CENTREVILLE, MD, March 20, 1872.

ART. IV.—*Plastic Apparatus in Surgery, with Especial Reference to that variety made with Plaster of Paris.* By SAMUEL B. ST. JOHN, M.D., of New York. (With two wood-cuts.)

FRACTURES afford the widest field for the application of the dressing which it is the design of this paper to advocate, and hence no apology is needed for a few general remarks on the past and present treatment of this class of surgical injuries. The literature of the treatment of fractures gives more conflicting advice than that of any other department of surgery.

"While more has been written on the subject of fractures than on hernia, lithotomy, amputation, or trepanning, yet no two books correspond, no two authors agree, even on the general points of practice, and every surgeon sets a broken limb, as he writes his name, after a fashion of his own."—*Bell, Princip. of Surg.*, vol. i. pp. 490–1. Lond. 1815.

Even so late as 1855 the question of immediate reduction was discussed, the negative being upheld by distinguished London surgeons. That question is now, happily, beyond discussion, and the controversy is upon the manner in which, and means by which, the reduction shall be maintained. If the ability of the medical or surgical man successfully to cope with disease or accident is measured by the paucity of the methods of treatment or varieties of apparatus with which the indications are to be met (and it surely ought so to be measured), who of us could expect a favourable result to follow the treatment of a broken thigh or a Colles's fracture of the radius? And yet we do expect it. Our egotism leads us to think that, by our shrewdness or acuteness of observation, or mechanical dexterity, we (perhaps we only) have been able to find the pearl in this heap of rubbish, to select from this mass of various, and it may be conflicting, advice, that which alone secures the desired end. Much more delusive, and more mischievous perhaps, because harder to abandon, may be a conviction founded upon one's own observation or theories.

Nevertheless, I boldly assert my belief that no other apparatus is so successful for treatment of fractures generally as the so-called immovable apparatus, and no other variety of that apparatus so good as that made with plaster of Paris or gypsum. This assertion compromises me to the utmost, for it is the expression of a conviction founded upon extensive observation and personal experience, and sustained by theories, some of which are original, so far as my knowledge goes.

Some variety of splint which shall restore in a measure the lost continuity and support of a broken limb, seems a suggestion almost instinctive, and hardly allowing of discussion. From the time of Hippocrates to 1768, splints of some kind, generally short wooden strips, were bound around a fracture with this object in view. In 1768 Percival Pott published a paper (a Few General Remarks on Fractures and Dislocations.

Chir. Works, vol. i. p. 221) in which he argued that the deformity, pain, and general distress attendant upon a fracture, were due *entirely* to irregular muscular action, and could be removed entirely by relaxing the muscles by position. His arguments were specious and forcibly conveyed, but they failed to secure the general adoption of his plan, for it was evident that he had lost sight of physical laws, which have so prominent a part in producing deformity and consequent pain; the distal end of a fractured limb falling by its own weight or failing to follow the proximal end, when that is moved either by accident or design. The muscular relaxation, too, it was seen, must be but partial, for the complete relaxation of a flexor group puts the extensors on the stretch, and *vice versâ*, so that a compromise, or semi-relaxation of both, is the best that can be had. Mr. Pott gives no statistics with his paper, showing results, but he says that this postural method "will not only succeed in all those cases in which the old method [by splints firmly applied] can ever be successful; but also in the majority of those in which it is not, nor, in the nature of things, can."

We notice also that he alludes to splints, which, according to Sir James Earle, he habitually used, and he insists upon having them extend over the joints above and below the fracture, and upon having them secured *loosely*, instead of firmly, as was then the general custom. It is easy to see that these long splints, though loosely bound on, would give more efficient support to a broken limb than the shorter ones then in vogue, be they bound never so tightly, so that Mr. Pott's *practice* was not such a revolution after all as at first sight it would seem. The probability is, that those cases which did well without any splints were those in which there was little or no tendency to displacement, and we confess to a disappointment in not having statistics of a number of cases of fracture of the femur giving the amount of shortening, where the treatment consisted simply in laying the limb upon a pillow in a flexed position.

Splints, therefore, still held their sway, and improvements were continually made in them as to lightness and adaptability to the affected limb. They were made of every conceivable material—straw, bundles of reeds or twigs, thin boards, sheet-iron, tin, wire, etc. Some of these were rudely moulded, to correspond to the contour of the limb, and the extension of this idea is to be found, at present, in all our instrument shops, in the shape of the elegantly carved and polished wooden splints, made rights and lefts like shoes, and warranted to fit any one who is patterned after the ideal Apollo of the maker. These splints, of course, required padding, elaborately arranged in order to make them support the fracture adequately, and to prevent too great pressure upon salient points, and the position of the pads was the prominent feature in the surgical instruction upon the treatment of fractures. These pads were continually slipping out of place, which, of course, rendered them worse than useless until replaced—thus

necessitating frequent reapplication of the whole apparatus, to the annoyance of the surgeon and discomfort of the patient.

Then came the era of an entirely new idea—new, as far as the surgeons of Christendom were concerned; but, like almost everything else worth knowing, it seems to have been known and practised by the heathen for centuries. This was to use some material which could be rendered temporarily flexible, or capable of being moulded so as to take the shape of any figure to which it was applied, and which, subsequently becoming inflexible and rigid, would preserve the imparted form. The distorted, deformed limb was then restored to its normal shape, and maintained there by an apparatus moulded over it—thus fitting a splint to the limb, and not, as had been too often done, fitting the limb to a particular splint. The originator of this idea is unknown, but whoever he may be, he made the most valuable contribution ever made to the treatment of fractures, and deserves the eternal gratitude of every sufferer from fractures, and of all surgeons who have fractures under their care, by enabling them to treat this class of injuries with more credit and less trouble to themselves, and more comfort to their patients.

The application of immovable apparatus to fractured limbs dates from a period too remote to be fixed with exactness. "The Greeks," says Pouqueville,[1] "used a kind of mastic to maintain fractures immovable, and the apparatus they left in place until the fracture united." Belloste in 1696[2] describes thus his application of an apparatus to a fracture of the femur with severe contusions: "As soon as he came into my hands, I made strong extension, reduced the fracture, and applied linen moistened with eggs beaten up with oil and vinegar . . . and placed the whole in a trough of pasteboard." This was allowed to remain for more than twenty days without being disturbed at all. A paper by Moscati[3] describes the application of an apparatus to a fracture of the humerus in 1739, which consisted in putting the limb in a kind of mould, "fabriqué sur elle méme," constructing "so to say, a kind of box which embraced the humerus and extended over the scapula and clavicle." This he made out of "bandages, oakum pads, and linen compresses," and covered the whole plentifully with the whites of eggs to bind together the pieces. Cheselden[4] gives the description of an apparatus formed of several pieces of linen saturated with a mixture of white of eggs and starch, and says, "there is no other bandage equal to it in the treatment of fractures." Sédillot[5] describes an apparatus in use among the Arabs, made of leather, pierced with rows of holes through which were drawn sticks of polished wood to give stiffness,

[1] A. Bérard. Archiv Gén. de Méd., Nov. 1833, p. 389.
[2] Le Chirurgien d'Hôpital, p. 330.
[3] Mémoires de l'Academie de Chirurg., vol. iv.
[4] Anat., 7th ed., Lond. 1750.
[5] Gazette Méd. de Paris, 1838, p. 135.

and says that these sticks were withdrawn to allow pus to exude, showing that they recognized the great advantages of an apparatus for compound fractures which did not require removal in order to dress the wound, and he says that "the Arab surgeons make the pressure more agreeable and more uniform by wrapping the limbs in moss, and they make their apparatus immovable by covering it with a layer of clay, which in drying acquires the solidity of plaster." According to Paperi[1] the nomadic tribes in Tunis used plaster or mortar, such as was used for walls, in the treatment of fractures. Sir George Ballingall[2] says the natives of India used moulds of potter's clay, and relates an instance of a compound fracture of the leg in a little boy, for which he was preparing to amputate the leg, when the boy was carried off by his parents to a neighbouring potter, who made a casing of clay in which the leg eventually was saved. Nannoni[3] speaks of the apparatus formed of white of egg and flour, and states that it was in common use among the Italian surgeons, though he does not recommend it himself. Roche[4] saw it in constant use during the Spanish campaign, white of egg chiefly being used, and left on as a rule until the fracture, whether simple or compound, had healed.

We see, therefore, that the idea of fashioning a splint out of flexible materials moulded to fit the limb is of very ancient date. But, with this, as with almost all other valuable inventions, there seems to have been a time when it was forgotten—when he who revived and methodized it, and gave it a definite place in science, deserves equal credit with its author. The reviver was Baron Larrey, who had extensive opportunities for experiments and observation in the campaign in Russia, and who in 1830 published a monograph giving the "results of twenty years' experience with it." And, indeed, his experience would seem to have been even more extended than this, for he speaks of using it in the Rhenish campaign of 1792–4.[5] For important modifications we are indebted to Baron Seutin, M. Velpeau, and M. Mathiesen.

As the surest indication of the advance or regression of any new plan or invention lies in the criticisms of recognized authorities of succeeding generations, and as by them its fate is, in a measure, controlled, no apology is needed for presenting in this connection the opinion of leading surgeons of the past century, especially as by a comparison we may notice not only the progress of an invention, but the progress *in* an invention, or improvement in the invention itself, and in the method of its use.

Petit (following Belloste)[6] recommends the use of wetted pasteboard

[1] Lettera al Prof. Barzellotti, Siena, 1821.
[2] Outlines of Milit. Surg., ed. 1852, p. 358.
[3] Trattato della Malatt. Chir., Pisa, 1793.
[4] Nouveaux Élémens de Pathologie Méd. Chir., 5th ed., Paris, 1828.
[5] Sur le Traitement des Fractures des Os, 1832.
[6] Traité des Maladies des Os.

splints "because they can be moulded exactly to the limb, and because in hardening they acquire sufficient solidity to sustain all sorts of fractures, and especially those which seem the most difficult to retain."

In 1837 the Russian authorities were so convinced of the superior merits of the starch apparatus that they ordered it to be adopted by the army and navy surgeons. Malgaigne[1] disapproves of it, but partly in a negative way, saying that he cannot see its advantages. Sir William Fergusson[2] gives qualified approval while admitting his lack of experience with it, and says, "I cannot but admit, that a fracture may be admirably treated in this way also. It is difficult, however, to imagine that the practice will be either safe or efficacious in all instances. It seems to be little in vogue among British surgeons." Pirrie endorses heartily the starch bandage after the subsidence of swelling, saying that he has had no experience in its immediate application, though he is inclined to think favourably of it.[3] Syme[4] does not think the "new method of starch bandage" of any advantage, and refuses even to admit that it is of advantage to allow a patient to leave his bed "to be propped up in a chair," but he betrays by his language an ignorance of the method of application and of its results. Druitt[5] approves of it, but says it should never be applied until all chance of swelling is over. Velpeau, in 1841, was so much pleased with it that he treated all his patients, having fractures, in this way. Bérard[6] used it freely, and relates with great gusto a case of simple fracture of the fibula which he allowed to go about on crutches on the 3d day after the accident. Cheselden[7] also praises the immovable apparatus. In a thesis of Dr. Markam in 1840 he says, "the starch apparatus, in the treatment of nearly all kinds of fractures, is now generally adopted by the Parisian surgeons, Lisfranc and Jobert alone excepted." Marchetti[8] states that "at the hospital of Grosseto alone, in three years (1842–5) Seutin's starch apparatus has been applied with *invariable success* in 14 fractures of the femur, 10 of the leg, 2 of the humerus, 8 of the clavicle, and 10 of the forearm." Erichsen[9] says "by employing the starch apparatus in the way to be pointed out, I scarcely ever find it necessary to keep patients in bed with simple fractures of the thigh for more than six or seven days, or of the leg for more than three or four,—thus saving much of the tediousness and danger." Hamilton[10] observes, "the cases to which this apparatus seems to be adapted are a

[1] Traité des Fractures et des Luxations, 1847, p. 263.
[2] Syst. Pract. Surg., p. 323.
[3] Princip. and Pract. of Surg., Lond. 1832.
[4] Princip. of Surg., Lond. 1842.
[5] Princip. of Surg., 1848.
[6] Archiv. Gén. de Méd., t. ii. 2me série.
[7] Operations in Surg. of M. Le Dran, trans. by Gataker, with remarks by Cheselden, 1768.
[8] Medical Statistics of the Tuscan Maremme.
[9] Science and Art of Surg., 2d ed., p. 182.
[10] Fractures and Dislocations, ed. 1860.

few examples of transverse or serrated fractures in which the bones have not become displaced, and in which little or no swelling is anticipated; and certain fractures which were originally more complicated, but in which a partial union, and the subsidence of the inflammation, have reduced them to a more simple condition; and especially is it adapted to cases of delayed union." The same author says, "I have even met with examples of compound fractures in which it has seemed proper to apply this dressing; but only when a sufficient time had elapsed to render it probable that there would be no sudden accession of swelling in the limb." The last author quoted has so far modified his views that, as he stated a few months ago, he is convinced that a fenestrated plaster of Paris splint is the best in which to treat nearly all compound fractures. "The objections to its use," Gross[1] remarks, "are altogether frivolous. The accumulated experience of the last twenty years is sufficient to convince any one of the safety and utility of this mode of dressing fractured limbs. It is not, of course, applicable to *all* cases, but there are few in which, in some stage of their progress, it will not be beneficial."

As we have already seen, the materials for making plastic apparatus have been very varied. The Greeks used "mastic" made of pounded shells or lime, white of egg, oil, and butter.[2] The Arabs used leather[3] and also plaster of Paris.[4] The natives of India employed clay,[5] the nomadic tribes of Tunis mortar. In the present century we find Larrey using a mixture of white of egg, camphorated alcohol, and extract of lead.[6] Velpeau, in 1830, used a mixture of ten whites of egg, ℥iv of lead-water, and ℥iij camphorated alcohol, but had previously used a glue of vinegar and rye flour. Seutin introduced starch, which was hailed by Velpeau as "une acquisition vraiment précieuse," but he (Velpeau) subsequently preferred dextrine "because the starch required heat, had to be applied with a brush or the hand, and did not readily soak into the bandages before their application." Also, because he thought it made a harder splint, and hardened more quickly. M. Chardon[7] recommends a mixture of white of egg, finely powdered alum, and lead extract, and in simple fractures also camphorated alcohol. In 1838, Lafargue de St. Emilion modified the starch apparatus by mixing with the starch powdered plaster of Paris, which possessed the quality of becoming "instantanement solidifiable." He gave this the name of the "gypso-amylacée apparatus," the proportions being equal parts of gypsum and starch. His contemporaries, however, seem not to have been impressed with the advantages of rapid consolidation, and it did not gain much favour. Pasteboard softened in water to

[1] Syst. of Surg., vol. i. p. 870. [2] Bérard, op. cit. [3] Eaton.
[4] Paperi, op. cit. [5] Sir Geo. Ballingall, op. cit. [6] Op. cit.
[7] De l'Etoupade d'Alun et de Blanc d'Œuf dans le Traitement des Fract., Bulletin Gén. de Thérap., Méd., et Chir., 1839, p. 355.

allow of moulding to the limb was used in the seventeenth century, and is spoken of by Belloste in 1696,[1] also by Bérard,[2] as being used by Assalini in Italy. Plaster of Paris began to assert its supremacy in 1852, and since that time only one other new material has been proposed, viz., silicate of soda, "soluble glass" or "water glass," which is recommended as being water-proof.

To sum up, then, regarding materials, we may say that leather, felt, pasteboard, and gutta percha—all of which may be softened in water—have always borne a high reputation. Liquid applications, to be applied with a brush or the hand upon a substratum of cloth, such as solutions of starch, glue, dextrine, or water-glass, have given a very brilliant series of results, and under this head may be classed that variety of the plaster of Paris splint made by soaking thick cloths in a plaster "cream" (made by mixing gypsum with water to the consistency of ordinary cream). The latest modification of this type of apparatus is made by using the dry plaster incorporated with the meshes of an ordinary bandage, as will be described subsequently.

With regard to the method of application of plastic apparatus generally, we may remark, that the earliest were made in a very clumsy manner by placing the limb in a box and pouring the plastic material around it, or by smearing it over the outside of the limb, as was done in those alluded to as being made of clay or mortar. The first advance was in wrapping the limb in some fabric, such as cloth or matting, which was smeared upon the outside. These methods secured a strong splint, it is true, if made thick enough, but exceedingly heavy and difficult to remove. A great improvement was made by soaking the cloths in the plastic material, made temporarily liquid by some menstruum—usually water. It was then found that equally strong splints could thus be made, which were much thinner and lighter than before, and the advantage thus gained by a combination of the fibre of the cloth with the resisting material led to a more intimate and uniform combination by making the splint out of roller bandage, saturated with the liquid starch, dextrine, or gypsum. An improvement, in my opinion, though not an essential modification is the use of the roller bandage having the plastic material rubbed in dry, as will be explained hereafter. In the mean time, a better idea of the methods of application will be obtained from the authors themselves.

Eaton, an English Consul at Bassora, in a treatise on the country of Syria,[3] describes the manner of application as he saw it among the Arabs:—

"A bad compound fracture of the leg from a falling field-piece, condemned to amputation by a European surgeon, was placed on oiled matting, the fracture reduced and plaster cream poured around it covering the posterior and lateral surfaces of the leg, foot, and part of the thigh. Pieces of hollow reed

[1] Op. cit. [2] Op. cit.

[3] A Survey of the Turkish Empire, London, 1798, p. 218.

were arranged to carry the discharges from the wound. After this had hardened the whole circumference was covered, furrows being made to allow of moistening. Horizontal and vertical grooves were then made in order to remove it easily when necessary."

This patient recovered completely in four months. This method is now known as "plaster packing," or "half-packing," when the limb is only half encircled, and is now almost entirely out of use. A distinguished surgeon of New York tells me that he saw it in use in Halifax in 1868.

Korzeniewski[1] describes an apparatus consisting of eggs, soap, and alcohol spread on clean flax, strengthened with slender splints and covered again with the mixture until it was an inch thick. Pirogoff[2] used stiffeners of linen strips soaked in the solution, laid both lengthwise and crosswise, which were "not to be put where fenestræ were to be cut," the whole then to be smeared over with the mixture. Lawrence, of Brighton (succeeding Cheselden), used a method described in Chelius's System of Surgery,[3] in which he moulded his plastic materials (cloth saturated with white of egg and flour) over one side of the limb, and, when that was dry, trimmed the edges, and, re-applying it, moulded the other half, leaving about half an inch to allow of tightening when required. This he used until he thought all danger from swelling was over, and then he put over all a starch bandage making a solid casing. Larrey's[4] apparatus was complicated; made of folded cloth, bundles of straw, pads of chaff and tow, compresses and a large piece of linen cut to fit the limb soaked in the white-of-egg mixture, the same being plentifully laid on outside. Seutin[4] at first used bundles of straw or strips of wood or iron as stiffeners, afterwards supplanting these by pieces of wetted pasteboard "*se moulant parfaitement sur toute la surface du membre fracturé*," and he simplified the apparatus of his predecessors by replacing the numerous pieces by simple linen rollers. He applied first a many-tailed bandage and placed (in fracture of the leg) alongside the tendo Achillis a small longitudinal splint, then a coating of starch, a second many-tailed bandage and another starch layer; two long side splints of wet pasteboard, reaching down so as to double under the sole of the foot, completed the apparatus. Seutin afterwards used the roller bandage soaked in starch solution. Lafargue,[5] thinking that the wet pasteboard stiffeners retarded the hardening, replaced them by iron wire. Seutin claims the idea of cutting up the splint so as to inspect the limb, and calls it his "appareil movo-amobile;" but Cheselden, in 1761,[6] recommends making the apparatus thin over the crest of the tibia "so as to facilitate cutting it up and tightening it when it becomes loose," so that he evidently practised it half a century before Seutin's time, and the successive steps would seem to be arranged as follows:—

[1] De Ossibus fractis tractatus, p. 496—quoted by Gürlt.
[2] Klinische Chirurg.
[3] Trans. by South., vol. i. p. 505.
[4] Mémoire sur le Bandage Amidonné.
[5] Gazette des Hôpitaux, 1839, p. 23. [6] Op. cit.

To the Greeks and heathen of unknown ages belongs the origination of the idea. To Baron Larrey the reviving and methodizing of the practice. To M. Seutin the use of starch and the extension of the principle to the treatment of deformities, diseases of joints, varices, luxations, etc., and the revival of the practice of Cheselden of cutting the splint longitudinally, which was called by Velpeau "*une des plus précieuses qualités de cet appareil.*"[1]

I have thus far avoided giving the modern history of plaster of Paris, because, that being the material with which nearly all the cases in which I used the apparatus were treated and that used in nearly all the cases employed statistically, it requires especial mention.

This, which has now to claim our attention, has the advantage of hardening more rapidly than starch, dextrine, or white-of-egg, of making a stiffer splint, and of being easily cut off when desired if applied in the manner to be described. Dextrine is very hard to cut, and starch is very sticky and requires stiffeners usually to give the splint sufficient firmness.

Plaster of Paris, the hydrated sulphate of lime, for surgical use must have been slowly heated under 400° F. to drive off the water of crystallization and must be kept in a dry place moderately well secluded from the air. When mixed with water it combines with it and forms a hydrate, or "sets," becoming in a few minutes quite firm and hard. If the surgeon suspects that his plaster has become deteriorated from exposure to the air, he may restore it to its former condition by heating it for a short time at a temperature of about 350.°

The use of gypsum by the Arabs has already been alluded to. It was first introduced into Europe by Hendriksz, who used it successfully at the hospital of Gröningen. In 1816, Hubenthal,[2] apparently without knowing of Eaton's publication, applied it to a fracture of the forearm by the "packing" process, but he divided the case in halves by a string laid underneath the plaster, and pulled up through it just before it became hard, as moulds are made now by those who make plaster images. Its formal introduction to the surgical world, however, as a material for treating fractures dates from 1828, when experiments were instituted at Berlin by Klüge, Dieffenbach, and Keyl. In 1831, Wm. Beaumont introduced it into England, and it seems to have enjoyed moderate favour until 1854, when Pirogoff published a monograph[3] giving an account of its use in the Crimean campaign, which gave a fresh impulse to experiments with this material. Mathiesen in 1854, published a monograph[4] in which he

[1] Gazette Méd. de Paris, 1858, p. 764.

[2] Nouv. Mapière de Traiter les Fract. Nouv. Journ. de Méd., tom. v. p. 210.

[3] Der Gypsklebeverband bei Einfachen und complicirten Knochenbrüchen und in seiner Anwendung beim Transport Verwundeter und auf dem Schlachtfelde.

[4] Du Bandage Plâtré et de son Application dans le Traitement des Fractures Liége, 1854.

advocated rubbing the dry gypsum into the meshes of a roller bandage, which is then to be rolled up, the whole to be moistened and applied like a dry bandage (*gypsverband*). This had the advantage of making an equally strong, though thinner and lighter splint, of being neater to apply, of applying itself to the limb more evenly and therefore of affording a more uniform compression, of being more susceptible of gradation of thickness to suit different limbs or different parts of the same limb, and finally, it could be more readily extended or pieced out should necessity arise.

In comparing a method of treatment of fractures with others, we should look at its results, its risks, its comfort to the patient, and its trouble to the surgeon, and perhaps, in the order given. For results I refer to the tables I have compiled, mainly from the records of Bellevue Hospital, taking all the cases I could find fully recorded, treated with this form of apparatus, being 187 cases of simple fracture and 26 cases of compound fracture. That there are risks attending it is not to be denied, but are there not risks with every form of apparatus? I can readily understand the unwillingness of some surgical instructors to advise this plan, though they believe in it themselves, knowing from observation, that this apparatus unskilfully applied and carelessly watched is productive of the most fearful mischief; but I am also convinced, that with skilful application and the use of the necessary precautions against accident, especially in the hands of one accustomed to the use of this form of apparatus, it is attended with less risks and gives better results than any other with which I am acquainted. Its comfort to the patient can hardly be considered under discussion, and the trouble to the surgeon is so much less than with other methods, that I was constantly in the habit of employing it when the case admitted of a doubt and I was pressed for time.

The *special advantages* may be enumerated as follows, but it should be remembered that we are here giving the advantages of gypsum splints, and that many of these belong to that variety in common with other moulded apparatus and not exclusively to it:—

Perfect coaptation to irregularities of the limb, and as result of this—	Little tendency to displacement of splint.	Less trouble to surgeon.
	Complete fixation of fragments.	Freedom of patient to go on crutches or otherwise. Less irritation. Less extravasation of blood. Less swelling.
	No injurious pressure over bony prominences.	Less liability of excoriation.
	Uniform compression, giving—	Muscular rest. Diminution of spasm and removing one cause of displacement, preventing swelling and lessening it when it exists.

Rapidity of hardening; allowing the surgeon to hold the fragments in

position until the splint will hold them; simplicity of materials; extensive range of application; painlessness of application; lightness; porosity; elegance; cheapness; readiness of piecing out if necessary without removing it; ease and accuracy with which the strength of different parts may be varied; allowing wounds to be dressed without disturbing the fragments.

Little Tendency to Displacement of Splint.—In the majority of cases coming under my charge, the splints did not need adjusting for weeks at a time, and here we note a point made by Déroubaix,[1] that the irregularity of the contour of the limb, instead of being a hindrance, is a positive help and guarantee for immobility.

Freedom of Patient to go about.—The routine plan of treatment of a simple fracture of the leg at Bellevue Hospital was to put it up immediately, and get the patient up on crutches the next day. I hardly think that surgeons of the present day will agree with Malgaigne in saying that "for the patient to leave the bed is of no therapeutic utility, by it the surgeon only aims at pleasing the patient, at dazzling the eyes by a kind of 'tour de force' which is not wanting in originality."[2] I take it, that on this point, argument is unnecessary. Regarding this point in connection with fractures of the femur, he says:—

"As to these, which the best constructed apparatus, with the aid of confinement to bed, so rarely succeed in curing without shortening, to add to the already existing difficulties of the fracture, the dangerous chances of walking, without even having to anticipate the slightest advantages therefrom, is a temerity which happily it is not necessary to combat, for I know of no French surgeon who has attempted it."

I refer simply to my tables, where are recorded fifty cases of this very injury treated with an average result better than that obtained by any other method, and in which, singularly enough, the two worst results are in cases which for other injuries were confined to bed during the consolidation of the fracture.

Less Irritation, less Extravasation of Blood, and less Pain.—That these results should follow a plan which prevents the ragged fractured ends from tearing fresh nerves and bloodvessels with every motion of the body seems rational, to say the least. In the cases coming under my charge, the splints as a rule not only caused no pain of themselves, but greatly diminished that existing at the time of application.

Less Liability of Excoriation.—The only two cases of severe excoriation which I saw, were cases of fracture of the thigh, one of whom under my care was *non compos mentis*, and did not call my attention to the fact that the apparatus was hurting him until the excoriation had progressed considerably. The other was a man who was very tolerant of pain, and

[1] Mémoire au Congrès Méd. Belge, presenté Sept. 1836.

[2] Traité des Fracts. et des Luxations, 1847, p. 63.

said nothing, supposing that it was to be expected that the splint would cause some pain.

Giving Muscular Rest.—The roller bandage has had great reputation in all times in the treatment of fractures, as preventing muscular contractions, so painful to the patient and so productive of displacement of the fragments. "One overcomes entirely and at will the power of the muscles by the uniform and circular compression of a well-applied bandage," says Larrey;[1] and Velpeau[2] also gives a powerful argument upon the advantages of uniform compression. This apparatus is virtually a *snug bandage* which cannot get *out of place*, and painful jumping of the muscles after simple fracture was, in my cases, almost unknown.

Prevents Swelling.—Not only that which comes from blood extravasated from vessels ruptured by movement of the fragments, but that which results from inflammation, following the continued irritation, if motion be allowed.

Lessens Swelling which already exists, by the acknowledged action of steady, uniform compression.

Rapidity of Setting.—In this differing from all other forms of moulded apparatus, so that the surgeon can be sure of retaining in position whatever he can hold in place for fifteen minutes. Some of the cases given in the tables, especially cases of Pott's fracture complicated with backward dislocation, could not be kept in place by any mechanical device, even for a few minutes, but the quick solidification of a gypsum splint solved the problem at once.

Simplicity of Materials and Cheapness need no comment. *Extensive Range of Application* will be considered further on.

Painlessness of Application.—On several occasions, in applying leather splints to a sensitive fracture, the force required to mould the leather occasioned so much pain as to compel a resort to anæsthesia, while in applying plaster apparatus I never resorted to anæsthesia except to relax muscular contractions. Its *porosity* is such that it permits transudation of perspiration, so that I have frequently seen the outside of splints moist from that cause, and this quality is exceedingly valuable in announcing superficial excoriations (should they not be discovered prior to this by pain) by transmitting the serum. In the same way I have known a new opening of a compound fracture disclose itself.

Ease with which Wounds may be dressed.—The ease to the surgeon, and comfort to the patient with which a wound may be dressed through a trap in a gypsum splint, needs no urging to any who have seen it used.

The dressing, instead of being anticipated by the patient as a most painful though necessary process, is, in many cases, but a grateful irrigation of the wound, unattended by pain or even discomfort.

[1] Clin. Chirurg., 1829, tom. iii. p. 425.

[2] Arch. Gén. de Méd., tom. iii. p. 210

I cannot coincide with Prof. Gross,[1] that "the *objections to its use* are altogether frivolous," knowing it to be powerful for evil, but I will express my conviction that those objections can be more easily overcome by a careful, judicious, watchful surgeon, than the objections to any other form of apparatus. They are as follow:—

Concealing Parts from View.—This is met by cutting the splint longitudinally so as to inspect the limb.

Constricting the Circulation.—This, the most weighty of all, is met in the same way, and we must remember that whoever bandages a limb, runs a risk of producing gangrene, but we do not, therefore, proscribe bandages. Gamgee[2] says of constriction and gangrene following the use of this apparatus:—

"In the numerous difficult cases which I have treated myself and seen others treat by this method, I never saw one of the accidents in question. I do not believe their occurrence possible in the hands of a skilful surgeon, except as one of those extremely rare, fortuitous occurrences which are apt to follow every operative procedure, and cannot be adduced as objections to a plan of treatment provided a sufficiency of well-established facts be adduced in its support."

Compression.—We have already seen the *benefits* of compression. It is charged that it prevents the formation of provisional callus, and delays the union.[3] The first I admit, the second I deny. Aside from the proven fact that some fractures unite with little or no callus, I noticed in cases under my observation, that there was a marked diminution in the amount of provisional callus, and frequently an apparent absence of it. With regard to union, as the tables will show, 51 cases of fracture of the tibia and fibula averaged 45 days in uniting; 27 of the tibia 42 days; 12 of the fibula 36 days; 12 of the humerus 37 days; 7 of the radius 31 days; 5 of the ulna 31 days; 19 Pott's fractures 39 days; 50 of the femur 44 days, etc. This is surely not long, and I have given tables showing the time of union in sixty-eight cases taken at random from the same records, where the limb was allowed to remain in a fracture-box, or loose splint, for at least one week, generally two or more; and attention is called to the fact that these cases compare most unfavourably with those treated from the beginning with moulded apparatus in just those fractures where the most motion is inevitable, viz., those of tibia and fibula, of tibia and of humerus. In experiments upon the lower animals, we find that if no splints are used, we get provisional callus; but using them we get none. It does not follow, however, that if we do part of nature's work she remains idle for a corresponding time. On the contrary, I believe that, finding the limb well supported, she goes immediately to work to make the permanent callus, and begins on the first week what is under other circumstances delayed till the fourth or fifth. On this point, Velpeau

[1] Op. cit. [2] Surgical Researches, p. 169.
[3] Malgaigne, Gazette des Hôpitaux, No. 7, 1858.

says,[1] "we understand, then, that the accomplishment of this end (l'union) will be the easier and the more rapid, the more perfect the quietude in which the fractured limb is kept." Who does not see, then, that, other things being equal, an apparatus which is never, or but rarely, renewed after its first application, is that which presents the greatest chance to insure this immobility so favourable to the consolidation of the callus?

Excoriation from Pressure has already been discussed, and applies only to those cases where the apparatus is so applied as to maintain extension. Even here it is less liable to excoriate than other forms, because it takes hold of a broader surface.

Becoming displaced when Swelling subsides.—This is fully met by the longitudinal section and taking in the slack.

Softening of Apparatus, if it is wet from urine, etc., especially in children. This may be averted by protecting it with oiled silk, or by making the splint waterproof, in exposed places, by a coating of varnish.

Œdema through traps left for dressing wounds. This will seldom give any trouble if oakum is firmly bound on, or if it does, it shows that the whole splint is too tight and needs section.

Special skill required in applying and managing it. This is a valid objection, and was so acknowledged thirty years ago by Velpeau.[2]

"To reap the good effects of the compression of the immovable apparatus in cases of fracture, I cannot too frequently repeat that the bandage must be applied in a very methodical manner, because, though compression well exercised is a truly heroic means in such cases, I must also warn you that when employed by incompetent hands, it may become the cause of more or less serious accidents. Never forget it, it is a powerful resource, but it will not admit of mediocrity."

The longitudinal section of the apparatus is a great safeguard against constriction, and I would recommend it to every one not practised in the management of this form of splint. Even with this precaution, there is danger which only intelligent watchfulness can surely avert. Minor objections have been raised, that this method is uncleanly; soils the bed, the surgeon's clothes, etc. This is exactly in proportion to the surgeon's unskilfulness. With the plaster bandage especially, it is possible to put on a splint without soiling the bed linen or clothes, if only care be taken to squeeze the bandage tolerably dry. Dr. Pierson[3] says that this apparatus causes by its pressure a painful affection of the nerves and loss of power in the muscles, so that the disability of the limb is more lasting than after other treatment. Richter[4] states that it arrests perspiration and thus causes superficial excoriation. I can only say that neither of these points is sustained by my experience. I cannot forbear adducing, at the close of

[1] Considerations Pratiques sur le Traitement des Fractures, Leçons Orales, p. 628.

[2] Leçons Orales du Clin. Chirurg., Bruxelles, 1841.

[3] Remarks on Fractures, 1840, Boston.

[4] Abhandlungen aus dem Geb. der Pract. Med. und Chir., Berlin, 1832.

this list of objections, the following cases to which frequent allusions are made in the literature of this subject, extracted from this Journal for Feb. 1840, p. 460. They all, with one exception, occurred at one of the Parisian hospitals.

Case 1.—Colles' fracture; apparatus too tight; produced gangrene of the hand. Careful watching of the state of the circulation should have been able to anticipate this. In all the cases in which it has been applied at Bellevue Hospital, so far as I know, there has been only one case of gangrene, which was in an out-patient who failed to report for examination, as directed, coming back only after the mischief had been done.

Case 2.—Compound comminuted fracture of leg. Apparatus applied two and a half months after to enable him to go about on crutches, which he did, and one day he slipped and fell, breaking his leg again, for which the apparatus were removed, when some abscesses were found under the splint. He eventually made a good recovery.

Case 3.—Fracture of leg; good union in forty days, but some deformity. The apparatus was not applied *till all swelling had gone*, and there was no apparent reason why the splint should not have kept the bones in place if it was well made and the fracture was thoroughly reduced.

Case 4.—Fracture of radius and ulna, united with deformity. The splint became loose and cotton was tucked in at the ends to tighten it, a proceeding which left it still loose in the centre. It should have been cut longitudinally, a piece taken out, and reapplied with a bandage.

Case 5.—Fracture of tibia; delayed union; no apparent cause. Malgaigne thought this case illustrated the effects of compression in preventing the formation of provisional callus.

Case 6.—Fracture of patella. Starch bandage applied without any back splint to keep the pressure off from the vessels. Patient seen "a few times," and "suffering no pain;" was not seen again for two months. Results as might have been justly feared—gangrene.

The plaster bandage of Mathiesen has lately received a thorough trial at Bellevue Hospital, and with the most gratifying results. It combines the firmness and solidity of the plaster with the tenacity of the cotton fibre, thus securing a firm and tough splint with a minimum of plaster, and, by consequence, a minimum of weight, with the additional advantage of application in a form which enables the surgeon to secure the most accurate adaptation to the limb under treatment. The plaster bandage is prepared by rubbing the fine, dry plaster into the meshes of a cotton or flannel bandage, the coarser the texture the better (within limits), as it holds more plaster. I have used also bandages of canton-flannel, and coarse linen (towelling). The linen ones made the toughest and lightest splint, and ordinary flannel next best. Lately tulle has been used, and it is highly spoken of as making a lighter splint than any other material, but it is too weak to be snugly applied. Tarletan was the material used extensively by the Germans in the late Franco-German war, and they strengthened their splint by their wooden ribbons, pieces of which were worked in longitudinally. A fine film of plaster is then dusted over it and the bandage rolled up rather loosely. These bandages may be kept ready for use in

tin cans with tightly-fitting covers, or each bandage may be separately rolled up in tin-foil. In making a splint for a fractured leg, for example, the leg is first enveloped in a layer of cotton wadding, or, what I prefer, a single thickness of soft blanket. If cotton is used it is best to hold it in place by a roller bandage, loosely applied over it. Burggraeve[1] preferred to use a layer of cotton one and a half inches thick, and no dry bandage, which he claimed was so elastic that it followed the diminishing leg as swelling subsided. The blanket will need no bandage, but may be secured by a few stitches at intervals. The plaster bandages are then immersed in a basin of hot water to which about ℥ij of common salt have been added (the high temperature and the salt both contributing to the rapid setting of the plaster). In about three minutes they will be saturated, and are ready to be applied. Extension then being made upon the foot, and the bones adjusted, the plaster bandage is taken from the basin, squeezed firmly to get rid of the superfluous water, and applied after the fashion of an ordinary roller, from the base of the toes up to or beyond the knee, each successive turn being drawn snugly to its place, but not too tightly, especially if the bandages have never been washed, as they will in that event shrink a little in drying. Experiments show that the plaster in setting does not contract at all, so that there is no danger of ligation from that source. Three, or at most four, bandages should make a firm splint from the toes to the knee. In applying the roller, should the plaster, which it holds, seem to dry, the hand may be dipped in the basin and rubbed over the dry surface, and indeed, it contributes to the firmness and elegance of the splint to keep the hand constantly passing over the successive turns of the bandage, rubbing in the plaster and smoothing the surface. An elegant finish and, at the same time, some additional strength may be given by mixing about ℥ij of the plaster with so much water and smearing it smoothly over the surface. It is best not to apply a dry roller outside, until the splint hardens enough to hold the parts in position, as it retards the setting by preventing evaporation, but the limb should be carefully held by the surgeon for a few minutes, and then placed upon a rubber blanket until it becomes quite dry and hard, when a dry bandage may be applied, if desired, though it is not at all necessary.

Thus far, the application in an average case of simple fracture has been considered. Several details, which will at once enter the mind of the surgeon, must now be considered. First. How soon may the splint be applied? Let us see what was the practice of our predecessors. Larrey,[2] as a rule, applied it immediately, except where there was any considerable swelling, or where the muscular contractions were increased by the unavoidable handling of the limb. Velpeau[3] says "apply at once in all cases where there is not swelling, or where the soft parts are healthy, for swell-

[1] Nouv. Système de Pansements Inamovibles, Bruxelles, 1838, p. 20.
[2] Op. cit., quoted by Gürlt. [3] Op. cit.

ing after fracture comes too soon for inflammation, and is mainly from extravasated blood, and an early reduction and complete retention of the fragments in place will prevent irritation, the initial step of inflammation, and prevent additional rupture of bloodvessels." Seutin (*op. cit.*) applied it at once, believing that it acted to prevent inflammation in still another way, the uniform compression of the bandage (according to his view) restricting the arterial, while it favoured the venous circulation. In a late article Volkmann[1] advocates immediate application, arguing that it tends to restrain traumatic reaction. For my own part, I believe that an intelligent surgeon, who can see his patient every day, and who employs the precautions alluded to, need not hesitate to apply the splint as soon as the patient comes into his hands. I speak advisedly in this, having repeatedly applied it while the patient was suffering from the shock of the accident, in injuries ranging from simple fracture of the fibula up to compound fracture of the femur, and in no case have I had occasion to regret doing so, but in all the cases the fixation of the irritating fragments has seemed to act as a prophylactic against swelling and general nervous irritation. In average simple fractures "constitutional symptoms" were almost unknown. These statements are supported by the cases given tabularly, where in eighty cases of simple fracture this splint was applied within twenty-four hours from the time of the injury, in many of them within two or three hours, and the fact that fourteen of these were from direct violence shows that cases were not selected which were less apt to be complicated by swelling. In twenty-six other cases the splint was put on within forty-eight hours, three being the result of direct violence.

Secondly. In what cases should cotton wadding be used as a lining, and in what cases blanket? A year ago I employed cotton where, from the contusion of the soft parts, great swelling might be anticipated; since I regarded it as a more elastic medium. At present I use blanket in every case, because it is more evenly distributed over the surface of the limb, and thus by giving more even support tends to prevent swelling, and also because by making an early section of the plaster casing it can be loosened a little should necessity arise.

Perhaps this reference to section of the apparatus will afford the most natural approach to an explanation of the manner in which it may be done, without hurting the patient or injuring the splint. This section may be most easily made about two hours after the application of the splint, as it then cuts like hard cheese. If, however, the section is delayed until the splint has become dry and hard, it may be softened in the track of the proposed section, by boiling water applied with a sponge for a few minutes. (Nitric acid is used by some for this same purpose, but it ruins the knife, sometimes goes through and blisters the skin, and does not make the section any easier.) A common shoemaker's knife, *kept sharp*, is the

[1] Berliner Klin. Wochenschrift, March 16, 1870.

best instrument to use in opening the splint, and the risk of cutting the patient may be reduced to a minimum by making two sections an inch apart, one on either side of the median line nearly through the plaster, and then raising the intervening piece at the top and completing the section by alternate nicks on either side. By thus removing a strip one inch wide a firm hold may be secured of the cut edge of the splint so as to spring it open to inspect the limb, and it also affords an opportunity of tightening the apparatus, should it have become loose by subsidence of swelling. Should the swelling, however, have gone down so much that it is necessary to remove a strip more than an inch wide in order to make it hug the limb, it will be better to remove the splints and make a new one, otherwise the tendency will be to roll the bones in toward the median line of the limb, in the case of the leg or forearm especially. I have tried various kinds of cutting pliers, Henry's, Seutin's, etc., and saws, also, but found none so serviceable as the knife. An instrument made by Leiter, of Vienna, a gentleman thoroughly versed in the management of gypsum apparatus assures me leaves nothing to be desired in this direction. Its mechanism is such that a tremendous leverage is obtained, so that when the lower blade is slipped between the splint and skin, the upper one cuts down upon it through the splint. The instrument has just been introduced into this country, and promises well, but I desire nothing better than a sharp knife.

In compound fractures the method of its application varies. If the opening through the soft parts is not large, and there is not much contusion or laceration, an apparatus like that for simple fracture may be applied, and a fenestra cut, through which to dress the wound. It must be made heavier and stronger than if the fracture were simple, but as the patient is to remain in bed with the limb supported for several days, if not weeks, this is not a disadvantage. In several such cases I found that five bandages were usually needed, and the splint was, in all cases of fracture of the leg, extended above the knee. The fenestra should be cut in accordance with the principles given in describing the longitudinal section of a splint for simple fracture. Some make a practice of leaving the piece covering the wound attached by one side like a trap-door, but in my opinion, it is neater to remove it entirely, as it enables the surgeon to trim the edges of the trap all around in a uniform manner, and moreover, the trap-door is apt to be in the way when the wound is being dressed. This method of procedure is, however, applicable only when the trap is to be of moderate size, of a breadth, perhaps, one-fourth of the circumference of the limb. The *length* of the trap is nearly immaterial, as it does not materially diminish the supporting power of the splint. When, from any cause, such as the extent of the external wound, or the suspicious look of the soft parts (and this applies equally to simple fractures with great contusion) it becomes necessary to have a large surface accessible to inspection without disturbing the fragments—a combination of the plaster bandage with blanket soaked in gypsum "cream" is to be used—and it was this

combination under the name of the "interrupted splint" which, during the past year, secured such brilliant and gratifying results at Bellevue Hospital in the treatment of bad compound fractures.

To apply this, the limb must be thoroughly inspected to find out what parts are least contused and least liable to become the seat of local trouble. It is only necessary to find a strip of sound tissue about two inches wide connecting the limb below the fracture with that above. If that can be found on the anterior surface of the leg or arm, so much the better, as the connecting band between the two parts of the splint will then be out of danger of being soiled by discharges, or of being wet when the wound is syringed out. A piece of ordinary blanket is then to be cut, about eight inches wide, and long enough to extend well above and below the joints on either side of the fracture. (In fracture of the leg it is usual to have this reach from the toes to the middle of the thigh.) The limb having been previously encased in blanket, as described for simple fracture, a plaster "cream" should be made, and the blanket previously cut out, immersed therein—or, which is preferable, the blanket may be laid upon a table and the mixture thoroughly rubbed in with the hand. It should then be folded upon itself twice so as to make a strip two inches wide, and laid along the part of the limb previously determined on. A plaster bandage should then be applied in the ordinary way below the point of fracture, binding the limb to the connecting piece and extending to as near the point of fracture as it is deemed safe to allow the limb to be concealed from view. Up to this point the limb has been supported by the hands of assistants, but no attempt made to reduce the fracture thoroughly. Now, the lower half of the apparatus being completed, an assistant, grasping it, makes strong extension (if necessary) and the bones are accurately adjusted. The upper end of the connecting band is then to be fastened to the limb above the fracture by turns of a plaster bandage. Thus an apparatus is made, fitting the limb accurately above and below the seat of injury, and having its two parts connected by a stiff bridge two inches wide—or, in other words, it is a plaster splint with an immense fenestra. In such an one which I made for a hopeless case of compound fracture of the leg, where the man refused amputation at first, the leg could be exposed from about three inches above the ankle-joint to two inches below the knee-joint, and the apposition of the bones which could be distinctly seen at the bottom of the wound, was perfectly maintained until the amputation two weeks later. In this case, owing to the oblique course of the crushing force (car-wheel), the only sound skin lay in a spiral line which I followed with the connecting band. If the connecting band is of necessity placed upon the depending side, it is of advantage to keep it away from the limb just opposite the wound by means of a thin blanket-pad placed under it; this pad to be removed after the splint has hardened, leaving a space through which a strip of oiled silk may be passed for the protection of the splint—the re-

mainder of the space to be stuffed with cotton or other material easily changed when soiled. Indeed the trap should be edged all around with oiled silk tucked under the edges, and cotton should be placed between the oiled silk and the skin to prevent the discharges from passing under the splint.

Another way which is very elegant, and which is of use in cases where the wound is not very large, is to cut, in a large piece of oiled silk, a hole a little larger than the wound, and then to fasten the edges of the hole to the skin around the wound by means of collodion. This collodion, being insoluble in the discharges, keeps the oiled silk so closely applied that the discharges are forced to find their way out through the hole in the silk, where they are received into a compress of oakum bound on. It will sometimes be found useful to paint the splint with a solution of gum shellac in alcohol (℥j to Oj) which gives it a water-proof finish. This, however, has two disadvantages; it prevents the transudation of perspiration, and it renders it somewhat difficult to enlarge the trap, should that be necessary, since it gives the surface a glazing which is extremely hard in itself and prevents the use of water to soften the splint in the track of the proposed section. In case the connecting band is found to be too weak, it may be at any time strengthened by additional strips of soaked blanket laid along it and secured above and below by turns of plaster bandage. In some cases where the connecting bridge was of necessity so narrow that I feared it would be too weak, I inclosed a stout piece of hoop-iron between the folds of the soaked blanket, the iron reaching about three inches above and below the proposed trap, the ends being imbedded in the more solid parts of the splint. This is also a useful plan to adopt when there is reason to fear that the trap might have to be enlarged and more strain brought to bear, consequently, upon the connecting band.

Special Fractures. *Fracture of the Femur.*—Perhaps in no other fractures have the merits of the gypsum apparatus been so conspicuous as in those of the femur, "it having triumphed here, not only by the brilliancy of the results, but by the comfort afforded the patient during convalescence." A simple fracture of the femur treated with any other form of apparatus compels confinement to bed for six weeks on the average, with the consequent lowering of vital status, discomfort to the patient, and trouble to friends and attendants; while with this the patient may be out of bed upon crutches the second or third day after the fracture, and is as comfortable and nearly as well able to wait upon himself during his whole convalescence, as is a man with simple fracture of the tibia in the fourth week of treatment under the old regime. And the results as regards shortening, time and strength of union, and subsequent usefulness of the limb, surpass statistically those of any other method of treatment, as a glance at the cases given further on will show. In the words of a surgeon whose name is identified with a different plan of treatment, "the statistics thus

far are better than those of any other method," and the refusal of a prominent teacher to recommend this method, is founded, as he himself acknowledged to me, on the belief that special experience is required to apply it properly rather than in any distrust of the method itself.

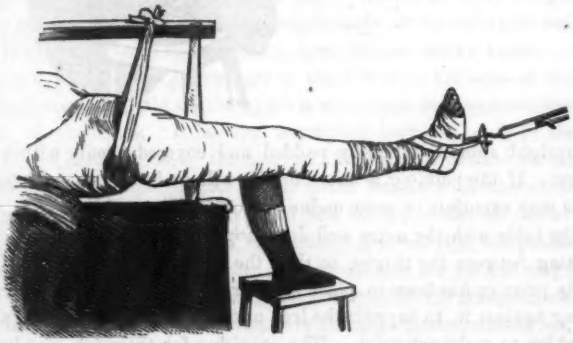
A description of the method of application is essential to the completeness of this paper, and I give it rather with that view than with the design of improving upon the admirable paper of my friend, Dr. Bryant.[1]

A table must be obtained and an upright bar about one inch in diameter secured to the middle of one end, so as to rise about two feet above the top of the table. In Bellevue Hospital an iron bar is kept which fastens on by clamps, but in private practice I have used with entire satisfaction, a wooden bar one and a half inch in diameter, put through a hole bored in the end of a common kitchen table and lashed to the braces below.

This upright should be thinly padded and covered evenly with a roller bandage. If the padding is too thick it will be in the way; if too thin the bar may excoriate or cause undue pressure. The patient is to be placed upon the table with the nates well down towards the edge, and the upright projecting between the thighs, so that the perineum rests lightly against it. My practice has been to place another table in line with the first and abutting against it, to support the legs of the patient and to afford a point from which to make extension. The provision for extension may be made in one of two ways, by traction upon a so-called Buck's extension apparatus, of adhesive plaster, which should be made with a very strong but narrow loop from malleolus to malleolus, so as to be in the way as little as possible, or, which is preferable, by a clove hitch around a stout plaster

[1] New York Med. Record, Sept. 15, 1871.

splint put on the day previous from the toes to the knee. A blanket previously spread upon the table should then be cut and drawn around the injured thigh so as to form a neat covering, and a strip eight inches wide should be extended around the pelvis. This should be drawn smooth and stitched as in a fracture of the leg. A strip of blanket one foot wide and four feet long (the relay), folded upon itself twice, so as to make a strip three inches wide, should be placed with its centre resting on the perineum and the ends brought obliquely upward and outward, the anterior passing over the crest of the ilium and the posterior over and above the tuberosity of the ischium. While the blanket is being adjusted, the patient may be etherized, and, as soon as fully relaxed, extension should be made, the scrotum (in the case of a man) having been carefully drawn upward and toward the sound side. The upright bar must be made to bear a little to one side of the median line to avoid injury to the urethra, and especial care must be taken that the blanket strip, called the "relay," should rest symmetrically in the flexure of the thigh and perineum, so as to present a groove along its center in which the upper edge of the inner side of the splint may rest. Extension is to be made by compound pulleys until the leg is by actual measurement as long as its fellow. If the lower half of the splint has been put on previously, as mentioned above, a trap may be cut over the malleolus to obtain a point from which to measure. Before applying extension strongly, however, the pelvis should be raised about four inches from the table by means of a stout sling, passing around the upper part of the pelvis and over a stout wooden bar stretching from the top of the upright to a stool, about two and a half feet high, placed upon



the table above the patient's head. Pillows should be placed under the head and shoulders to elevate them to a line with the pelvis. The bandages are now to be applied, piecing out the part already on and completing the splint as you pass upward. Pains must be taken to make a neat spica and not to allow the "relay" to become displaced. Small pieces of blanket soaked in plaster "cream" may often be used with advantage to strengthen

the anterior and antero-lateral portions of the upper part. Felt or pasteboard may also be used for this purpose, being worked in between the layers. About ten bandages will be needed in all, including those used in making the lower half. After the application is complete, the patient should be lowered to the table, but the extension should be kept up until the splint is quite hard. The projecting ends of blanket should then be cut off and the perineal edge trimmed with oiled silk to prevent injury by wetting, especially in children.

In Vienna, the upright used for counter-extension has a projection from its side, about six inches from the top of the table, on which the tip of the sacrum is made to rest, the body and upper lumbar region being supported by a frame about eight inches high which rests upon the table. In one of the cases given in the table (Case No. 23), Dr. Bryant used no upright for counter-extension, but made traction instead upon the "relay" in the perineum. This idea has been still further practicalized by Dr. Van Wagenen, at present connected with Bellevue Hospital, by passing counter-extending bands over the perineum and having brought them up one on each side, fastening them to a frame upon which the patient lies, which frame raises his body a few inches from the table, while his pelvis projects beyond its edge so that the hands of the surgeon can readily pass between that and the table.

In its application to *fractures of the tibia and fibula*, there is nothing especial to be said except that, these being the most common fractures, this method has here received the most thorough trial. Three good bandages (five yards long) will make a stout splint for most adult legs, unless the fracture should be near the knee, when four may be required, as it is then essential to carry the splint half way up the thigh, and it should in all cases extend to the toes when first applied to insure quiet of the leg muscles. In cases of high fracture of the tibia, it is often advantageous to make a circular amputation of the splint above the ankle after two weeks and begin passive motion of the ankle-joint. This is preferable to removing the leg temporarily from the splint. When there is great tendency to overriding, a good plan is to flex the leg upon the thigh sufficiently to enable the posterior surface of the thigh to act as a surface of counter-extension, the splint being carried above the knee. In Pott's fracture I have used with satisfaction, to produce inversion of the sole of the foot, a Dupuytren's splint applied outside the splint to give it shape while hardening, obviating the necessity of holding it, which may be tedious if the plaster is poor.

One case of Pott's fracture under my charge, complicated with backward dislocation of the foot, was so admirably maintained in position by a gypsum splint as to elicit the commendation of an avowed opponent of the system, who said he could not think of any other apparatus which would fulfil all the indications. When it is desirable to have the lightest possi-

ble splint, pasteboard, gutta-percha, or felt may be worked in between the successive layers of bandage, and they will be held so tightly as to be in no danger of slipping even if the splint be entirely removed and replaced. Two strips of felt and two plaster bandages thus disposed make an elegant, effective, and very light apparatus. The use of alum water (℥iv to Oij) in which to soak the bandages renders it possible to make a stiff splint out of two good plaster bandages, but in my hands they have proved brittle, not having elasticity enough to allow of their being sprung off (should there be occasion) without damaging them. Such splints, however, answer admirably after being cut down not only in front, but also posteriorly, leaving only two points of attachment of the two halves, one at the heel, the other at the top, so that the two halves were merely hinged together. The edges of these splints for the leg may be bound with adhesive plaster, so as to prevent their being broken by the surgeon's fingers in springing them open to inspect the leg, and this contributes much to the elegance of their appearance.

In a few cases of *fracture of the patella*, the plaster bandage has been used with remarkable success; in no case, so far as my knowledge goes, with a decidedly bad result. When the joint is tense with effusion, it cannot well be applied, but if there be only a moderate amount it may be used, and indeed, has been, with such a close union of the fragments that it was pronounced bony union. In these fractures, as there is usually little or no contusion, a roller bandage may be substituted for the blanket lining. Two firm compresses having been laid, one below the lower fragment, the other above the upper one, and secured by strips of adhesive plaster, the plaster bandage is then applied from the ankle up, making a figure of 8 around the knee and continuing the splint well up toward the groin. This should be applied while the patient sits on the edge of a table or chair to relax the extensors. Some place behind the popliteal space a light wooden splint, about four inches wide and a foot long, so that more tension may be used in drawing the fragments together by the figure of 8 turns without danger of constricting the limb. It will be seen that the summit of the knee may thus be left exposed for examination of the fracture, and it seemed, in two cases falling under my notice, that there was a decided tilting forwards of the fractured ends from the strong backward pressure of the bandage upon the other extremities of the fragments. This, it was claimed, could be counteracted by carrying additional turns of the bandage directly across the line of fracture, but it has the disadvantage of hiding the fracture completely from view. The number of cases is not, however, yet sufficiently large to enable one to cover up such a fracture with confident expectation of a good result. A compromise may be made by treating the fracture through a large fenestra cut in a stout plaster splint reaching from ankle to groin, which forms a posterior splint incapable of displacement, securing rest of the quadriceps extensor muscle, and

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affording very convenient points of attachment for straps, strings, or adhesive strips, or whatever one may be using for the approximation of the fragments. In that plan of treatment, which consists in drawing the fragments together by strips of adhesive plaster crossing each other, this form of splint is especially useful, since traction can be made directly downward and the strips fastened to the front of the splint by a few turns of a dry roller, instead of passing downwards and *backwards* as in the ordinary posterior splints, and tilting the fragments forward as already alluded to. If, as some hold, the separation of the fragments is due mainly to effusion into the joint, the success already attained by the use of the bandage alone may be due to pressure stimulating absorption.

In simple *fracture of the ulna* it has in several instances proved itself to be all that could be desired. But as this is not a severe test of its powers it may be passed by with the remark that those who believe in interosseous pads for fracture of the forearm can mould a plaster splint before it hardens by indenting it firmly along the interosseous space, so as to make a prominent ridge when viewed from the inside. For convenience in allowing a coat sleeve to be worn, it is certainly admirable, while impatient but imprudent patients, who would rebel against wearing clumsy wooden splints, by way of precaution, after a fracture had united with tolerable firmness, will readily submit to a light and comfortable gypsum one.

In *Colles's fracture of the radius*, its advantages over other methods are not so evident. Still its record here is by no means a bad one. I succeeded in two cases in obtaining excellent results by using the gypsum bandage from the elbow to the wrist-joint, making the splint especially strong and thick at its lower edge, and holding the fracture reduced by pressure upon the splint until it was quite hard. In one case I used small pads upon the radial and ulnar borders of the lower part of the forearm, so that the splint could be more firmly applied without risk of arterial and venous obstruction. Of course the fingers and hand had to be bandaged with dry rollers. In fractures of the radius high up, the recorded cases are all in its favour. In fracture of both bones with tendency to shorten, counter-extension should be taken from the anterior surface of the upper arm, the forearm being flexed at a right angle and the splint extending about three inches above the elbow-joint.

In *fractures of the humerus* low down the forearm should be flexed at a right angle, and the splint applied from the wrist to the axilla. If there is much shortening, two methods are in use, one that just given, relying upon the weight of the splint for extension; the other where the splint is extended over the shoulder, forming a shoulder-cap, by means of the spica, which closely hugs the axilla and takes its counter-extension from that part. It is claimed by some, that any effective counter-extension from the axilla would check circulation through the axillary vessels, but evidences of serious obstruction complicated none of the cases in which I applied it

although the upper edge of the splint was well imbedded in the axilla. The counter-extension was derived, it seems to me, mainly from the angle of the axillary border of the scapula with the long head of the triceps muscle, the scapula being tolerably well fixed by the spica and a body bandage.

In *fractures of the clavicle* it is not more successful than the usual treatment in Bellevue Hospital at present, by two strips of adhesive plaster, one around the arm and passing back, and the other under the point of the elbow and over the opposite shoulder. Applied in the ordinary way in which a roller bandage is used by some in this fracture, it is superior, inasmuch as it retains its place, the successive turns not sliding upon each other, but it is most serviceable as an auxiliary to the adhesive plaster strips, to distribute pressure, especially over the olecranon process of the ulna. I have twice seen œdema and commencing ulceration in the opening cut to avoid pressure over the tip of the olecranon process, the risk of which can be entirely avoided by the use of a little gypsum cap moulded over the elbow, a small pad (to be subsequently removed) making a depression which receives the olecranon.

So far as I have been able to learn, the plaster bandage has been applied only once in *fracture of the ribs*, and that was in the last case of this injury which came under my care during my hospital service. It occurred to me that it might possess two advantages over the adhesive plaster bandage, viz., less liability of slipping, and a certain amount of vertical stiffness not possessed by the adhesive plaster, the gypsum apparatus formed by successive turns of a three-inch roller sinking into the intercostal spaces and preventing the motion of the fragments under the spasmodic action of the bruised and irritated intercostal muscles, while it prevented the motion of the whole sterno-costal system equally well with the adhesive plaster bandage. The success of this single trial was all that could be desired, and the relief was more rapid and more complete than I have ever seen with the adhesive band.

In one case of *fracture of the inferior maxilla*, where a gypsum cap was made to fit the chin and was bound on with a four-tailed bandage, it seemed to me to be inferior to gutta-percha, being rather heavier and more clumsy.

In the manufacture of apparatus to fulfil certain indications after exsections, much opportunity is given for the exercise of mechanical ingenuity, and much independence of instrument-makers may be gained by the surgeon. Thus, in resections of the knee, elbow, or ankle, an apparatus may be moulded to the part previous to the operation, taken off and reapplied. This should be of the "interrupted" variety previously described, or, if desired, it may be "bracketed," that is, one where the upper and lower parts are connected by two or three iron brackets imbedded at either end in the solid plaster portions. An apparatus for use after an exsection

of the hip in a boy four years old has received such an unexpected amount of credit from surgeons who saw the case, and illustrates so well the variety of indications that may be met without expense of time, money, and patience at the instrument-makers, that I give it somewhat in detail.

The boy being placed, face downwards, upon a hard bed, and moderate extension being kept up on the leg operated on, a blanket was first thrown over his back and legs, as a lining for the apparatus. A blanket soaked in gypsum "cream" was then moulded over his back and legs, reaching down to the bed on either side, and between the legs. Strips of stout hoop-iron were laid upon this to serve as stiffeners or braces, and, additional pieces of soaked blanket being laid upon them, the edges of the first-mentioned piece were turned back over them and the blanket lining turned back over all. This hardened in a few minutes, was removed, trimmed, and a large opening cut to allow of defecation and of dressing the wound. Thus a mould of the posterior half of the body and legs was obtained, from the armpits down. The trough corresponding to the sound leg was turned up at its lower extremity to make a foot-piece, but the other was extended in a semicylindrical shape about six inches beyond the heel, and across its open extremity stretched a bent iron strip whose ends were imbedded in the plaster on either side. This served for the attachment of an India-rubber loop for keeping up extension by pulling upon a so-called Buck's extension apparatus applied to the leg. The little fellow, when bandaged into this was as portable as a log, and was taken out daily upon the balcony when the weather permitted. For more than two months he lay bound to this trough, and yet so evenly distributed was the pressure of his body that there was not even excoriation over bony prominences, although no padding of any kind was used; while another boy, in better condition, under observation at the same time, who had been subjected to the same operation and placed in a wire splint, developed two very troublesome bedsores in spite of all that I could do by padding and stuffing.

This immunity from danger in pressure upon a hard surface when the pressure is evenly distributed suggested to me the applicability of a gypsum apparatus in cases of threatened or actual bedsores, pressure being removed from the painful spot by cutting a hole or making a depression at that point. I have had only one opportunity of testing the practicability of this plan, and the result in that case was indecisive, the patient being at the time moribund. I am encouraged, however, by this case to repeat the experiment, and if successful it will be a cheap substitute for the air- or water-cushion, and not so readily displaced. In one case I applied a gypsum apparatus, at the instance of another surgeon, as a brace in incipient caries of the spine; but was not able to follow up the case.

Rest for inflamed or luxated joints is admirably secured by an apparatus like that for simple fracture, and any desired applications or inspection can be made through a large trap. Extension can be combined with rest upon the principles already given when treating of extension in fractures with shortening.

In contrivances to equalize pressure gypsum is invaluable. In a case of upward dislocation of the acromial end of the clavicle, a gypsum apparatus

extending from the wrist to the middle of the humerus (the arm being flexed at a right angle), fixed the forearm and distributed pressure over its whole ulnar side in such a way that an adhesive plaster strip carried under the forearm and over the clavicle held the bone in place for weeks without pain or inconvenience. The fact, that in this case, before the application of the gypsum apparatus, the pressure of the strap was so unbearable that it was necessary to take it off at the end of twenty-four hours, shows its utility.

A gypsum splint from the toes to the lower ring of a "Sayre's extension apparatus" for the knee-joint, proved very useful as a substitute for the adhesive plaster strips around the lower ring, and suggested itself to me by the continual slipping of the strips, necessitating reapplication of the whole apparatus. The pressure in these cases was evenly distributed over the dorsum of the foot, and caused no pain or excoriation. It will be seen that the extension was here maintained by pushing on the foot instead of pulling on the leg, as is done by the adhesive strips.

It has proved useful, also, as a temporary precautionary apparatus, applied to sound limbs upon which, for some reason (such as reduction of luxation, etc.), unusual strain was brought to bear. In case of a fracture of the humerus below the middle with luxation at the shoulder-joint, an apparatus of this kind might enable the reduction to be accomplished without waiting for the fracture to unite. In such cases, the fingers having been firmly bandaged, the splint should be applied very tightly, and after the reduction at once cut up longitudinally and loosened a little. Under this head of precautionary apparatus would come the splint mentioned in Case 10, further on, which was used where there was necrosis following a compound fracture, the sequestrum becoming loose before the fracture had united with sufficient solidity to allow of an operation unless the parts were thoroughly supported, and in which I removed through the trap a large sequestrum which, but for the splint, it would have been unsafe to meddle with, for several weeks.

I subjoin the statistics of 142 cases of simple fracture, mostly taken from the records of Bellevue Hospital, which were treated by the gypsum apparatus.

Of these 142 cases of simple fracture, there are 51 of the tibia and fibula (exclusive of Pott's), 27 of the tibia, 12 of the fibula, 12 of the humerus, 7 of the radius, 5 of the ulna, 19 Pott's fractures, 1 of the astragalus, 2 of the os calcis, 1 of the scapula, and 5 of the patella. The average time of union of the tibia and fibula was 45 days. 41 cases averaged only 36 days. The average time of union of the tibia was 42 days, and 22 of these averaged 38 days. Of the fibula, the average time was 36 days; of the humerus 37 days; of the radius 31 days; of the ulna 31 days; of Pott's fractures 39 days; of the ossa calcis 30 days.

Of the fractures of the tibia and fibula, 22 were put up in the plaster splint within the first 24 hours, generally within the first 12 hours; 4

were put up between 24 and 48 hours afterwards; 15 between 2 and 7 days, and 10 after that time. Of the fractures of the tibia alone, 11 were put up within 24 hours; 2 between 24 and 48 hours; 10 between 2 and 7 days, and 4 subsequently. Of fractures of the fibula alone, 4 were put up within 24 hours; 1 between 24 and 48 hours; 3 between 2 and 7 days, and 1 subsequently. Of fractures of the humerus, 7 within 24 hours; 1 between 24 and 48; 3 between 2 and 7 days, and 1 subsequently. Of fractures of the radius, 5 within 24 hours; 2 between 2 and 7 days. Of fractures of the ulna, 2 between 24 and 48 hours; 2 between 2 and 7 days, and 1 subsequently. Of Pott's fractures, 8 within 24 hours; 2 between 24 and 48 hours; 6 between 2 and 7 days, and 3 subsequently.

In all, 57 fractures were put up within 24 hours after the accident, and of these 8 occurred from direct violence, by which is meant heavy bodies falling upon them. 12 were put up between 24 and 48 hours, of which 1 was from direct violence; 45 between 2 and 7 days, of which 8 were from direct violence; 23 after the first week, of which only 1 was caused in that way.

The whole 142 cases average 40 days in uniting. Sixty of these cases were treated in wards with which the author was officially connected, as senior assistant and subsequently as house surgeon, and in a majority of these the apparatus was applied and the cases were watched by him personally, and care has been taken to verify the correctness of the hospital statistics of some of the most important cases by comparison with private records.

Not to confine my cases entirely to those treated with the gypsum splint, I have compiled the following cases, comprising, with those already given, all the simple fractures (exclusive of fractures of the femur) treated at Bellevue Hospital within three years, of which a full record is found on the books. The compilation is mainly with reference to the time of union.

First.—*Fractures treated with Leather, Felt, Pasteboard, or Gutta Percha Splints.*

24	cases	fracture of	tibia and fibula	averaged	44	days	in uniting.
15	"	"	tibia	"	37	"	"
9	"	"	fibula	"	43	"	"
27	"	"	humerus	"	39	"	"
18	"	"	radius	"	34	"	"
15	"	"	ulna	"	32	"	"
13	"	Pott's	fracture	"	37	"	"

Second.—*Cases treated for at least one week, generally two, with Fracture-Box, or other loose Splint.*

31	cases	fracture of	tibia and fibula	averaged	50	days	in uniting.
12	"	"	tibia	"	47	"	"
6	"	"	fibula	"	38	"	"
10	"	"	humerus	"	42	"	"
1	"	"	ulna	"	35	"	"
8	"	Pott's	fracture	"	45	"	"

Of the 1st class we have 121 cases, with an average time of union of 38½ days. Of the 2d class, 68 cases, with an average time of union of 47 days.

Table giving 50 Cases of Fracture of the Femur treated with the Gypsum Apparatus.

Name.	Age.	Date of fracture.	Point of fracture.	Am't of shortening.	Date of application of apparatus.	Date of removal of apparatus.	Am't of shortening after union.	Cause of fracture, and remarks.
				Inches.				
John McN.	7	Jan. 13	Junction up'r & mid. thirds	....	Jan. 15	Feb. 23	None	
Pat. R.	37	" 7	Lower third.	½	" 9	" 23	None	
John W.	33	....	Below trochanter.	1½	Splint on	7 weeks	¼	Cart-wheel. Ether not used.
Ed. M.	3	....	Middle.	1	" "	45 days	¼	Kick of horse. Ether not used.
Frank P.	33	....	Lower third.	1½	" "	7 weeks	¼	Direct violence. Ether not used.
Ed. M.	14	....	Middle "	1½	" "	42 days	¼	Ether not used.
Pat. D.	19	Nov. 20	Upper "	1½	Nov. 28	Jan. 6	None	Direct violence. No ether.
James H.	45	Jan. 13	Middle.	1¾	Jan. 13	Mar. 1	¾	Fracture compound.
Adam B.	43	Dec. 15	"	1	Dec. 16	Jan. 21	¼	Ether used.
David D.	63	Nov. 3	Cervix.	1½	Nov. 7	Dec. 19	¾	Ether used.
Peter O.	60	" 24	"	1½	" 29	Jan. 14	None	Ether used.
Charles S.	55	" 12	"	1½	" 16	" 7	¾	Ether used.
Thomas F.	16	Oct. 22	Middle.	1	Oct. 25	Dec. 5	¼	Ether used. Direct violence.
Pat. M.	33	" 15	"	1½	" 16	Nov. 27	None	Ether used.
Wm. M.	4	July 19	"	¾	July 20	Aug. 25	¼	Ether not used. Walked on splint.
Pat. F.	28	" 24	Lower third.	....	" 25	Sept. 2	None	Ether not used.
Wm. S.	9	" 22	Middle.	....	" 23	Aug. 28	None	Ether not used. Direct violence.
Benj. R.	1½	Aug. 11	"	....	Aug. 12	Sept. 13	None	Ether not used. Direct violence.
John M.	10	Apr. 21	"	....	Apr. 28	June 2	¾	Ether not used.
Ann H.	28	Mar. 2	"	1½	Mar. 3	May 14	¾	Ether used. Fracture compound, and from direct violence.
Peter R.	29	Apr. 12	"	1¾	Apr. 14	" 26	None	Ether used.
Pat. M.	38	" 26	"	¾	May 6	June 24	None	Ether used.
Lewis G.	3½	May 21	Lower third.	....	" 21	" 29	¼	Ether used. Patient walked on splint.
Conrad C.	13	June 6	Upper "	1	June 8	July 22	None	Ether used. Direct violence.
John W.	5	" 13	Middle.	1	" 14	" 20	None	Ether used. Direct violence.
John K.	32	July 14	Lower third.	1½	July 18	Aug. 30	¼	Ether used. Wheel of truck.
James B.	35	" 27	Middle.	....	" 28	Sept. 8	¼	Ether used.
John M.	27	Oct. 5	Lower third.	....	Oct. 10	Dec. 1	¼	Ether not used.
Nellie W.	4	Aug. 20	Middle.	1	Aug. 21	Sept. 20	¼	Ether used.
James B.	35	July 27	"	....	July 28	" 15	..	Ether used.
James C.	32	Oct. 12	Lower third.	1	Oct. 13	Dec. 6	¼	Fracture compound.
Frank A.	35	Aug. 7	" "	1	Aug. 16	Oct. 1	¼	
Fred. B.	45	June 22	" "	....	June 24	July 31	..	Good union and position.
Mat. B.	4	Apr. 26	Upper "	....	Apr. 22	May 15	..	Good union and position.
David D.	59	May 16	Lower "	....	May 17	June 26	¼	
Thomas L.	6	Apr. 17	" "	1½	Apr. 19	" 1	None	Ether used. Fracture by direct violence. Splint cut longitud'y.
Mark W.	28	" 14	Upper "	2	" 15	" 6	None	Ether used.
John S.	..	Mar. 17	Lower "	2	Mar. 18	" 15	None	
Samuel L.	18	Nov. 11	Middle.	1	Nov. 13	Dec. 16		
John R.	55	Sept. 14	Cervix.	1	Sept. 18	Oct, 23		
Joseph C.	48	Aug. 22	"	....	Aug. 22	Sept. 20	1¼	Patient in bed during treatment.
Mary B.	50	June 23	Cervix int. cap	1	June 25	Aug. 5		
John M.	32	Dec. 13	Upper third.	....	Dec. 14	Jan. 16	1	
George W.	5	Nov. 30	Middle.	....	" 1	July 6	1	
Wm. S.	43	May 28	"	....	.....	" 6	1	Patient in bed during treatment.
Fred. B.	45	June 22	Lower third.	Marked.	June 24	" 29		
John L.		Nov. 14	" "	....	Nov. 14	Dec. 29		
John McD.	7	Feb. 1	" "	½	Feb. 3	Apr. 10	None	
M. C.	..	Nov. 23	Middle.	Marked.	Nov. 25	Jan. 2	None	
John C.	58	Dec. 7	Lower third.	....	Dec. 7	" 24	¼	
Pat. C.	23	June 21	Middle.	2	June 24	Aug. 20	1¼	Patient in bed during treatment.
Jas. D.	50	Apr. 23	Upper third.	1½	Apr. 26	June 23	None	

In analyzing the cases we find that the average shortening in all 50 cases is $\frac{1}{3}$ of an inch. The average shortening before treatment, so far as given, is $1\frac{1}{4}$ inch. The use of an anæsthetic does not seem to have essentially altered the result, for 38 cases where ether was used give an average shortening of $\frac{1}{3}$ inch. 35 of these, where patients went about freely on crutches, give average shortening $\frac{1}{4}$ inch. The average time of union in all 50 cases to time of leaving off splint is 44 days. 18 cases give no shortening, the average shortening of these 18 on admission was $1\frac{1}{3}$ inch. 16 cases under 18 years of age give $\frac{1}{8}$ inch shortening. 27 cases over 18 years give between $\frac{1}{3}$ and $\frac{1}{2}$ inch, and 21 of these average less than $\frac{1}{4}$ inch. In 23 cases the splint was applied within 24 hours; of these 6 were from direct violence, and 2 were compound. In 14 cases the splint was applied between 24 and 48 hours after the injury; of these 2 were from direct violence. In 8 cases the splint was applied between 2 and 7 days after, and in 3 cases after 7 days had elapsed.

	Average Shortening.	On Admission.
21 cases had femur broken in middle	$\frac{1}{4}$ inch.	$1\frac{1}{4}$
15 " " " junction of middle and lower third	$\frac{1}{3}$ "	1
7 cases had femur broken in junction of middle and upper third	$\frac{1}{16}$ "	$1\frac{1}{4}$
5 cases had femur broken at cervix	$\frac{1}{2}$ "	$1\frac{1}{4}$
1 case had femur broken below trochanter	$\frac{1}{4}$ "	$1\frac{1}{4}$

How do these statistics compare with those of other modern hospitals? Holthouse, in the last edition of Holmes' Surgery, says that at present, in London hospitals, shortening occurs after fracture of the femur in adults in 90 per cent. of the cases, and 50 per cent. in children. In these cases it is 25 per cent. in adults, and 50 per cent. in children. He gives 50 cases with an average shortening of $\frac{3}{4}$ of an inch. He recommends the plastic apparatus for infants, and for adults after the *first six weeks*. A paper by Dr. Gurdon Buck, of this city, in 1861, gives 20 cases, 17 of which had from no shortening up to $1\frac{1}{4}$ inch; average, $\frac{1}{2}$ inch; only 1 adult without shortening, and 3 children. In a number of cases collected by Dr. Shrady from the Records of the New York Hospital in 1859, out of 74 cases 19 had no shortening; 55 had $\frac{1}{4}$ inch. Of the 19, 13 were over 12 years old. Average shortening of the 11 under 12 years of age was $\frac{1}{2}$ inch. In 27 cases reported in *New York Journal of Medicine*, 1858, by Mr. J. Campbell, the average time of union was 45 days.

In every point, then, are the statistics of cases of fracture of the femur treated by this method superior to those of any other method. It seems to me idle to say that the patient runs too many risks of falling, etc., if he is allowed to go about on crutches. Surely he runs risks if confined to a close room, or the foul air of a hospital ward, when he might be breathing the pure air outside. The difference seems to be that if he falls and refractures his thigh it is chargeable to the stupidity of his surgeon; if he dies from pneumonia, empyema, or an enormous bedsore over his sacrum, it is a "visitation of Providence."

Table giving Statistics of 26 Cases of Compound Fracture treated with Gypsum Splint.

No. of case.	Name.	Age.	Date of fracture.	Bone and Point.	Date of application of apparatus.	Date of removal of apparatus.	Result.	Remarks.
1	Jno. T.	22	Oct. 18	Tibia, middle	Oct. 19	Nov. 27	Good union and position	Fractured by direct violence.
2	A. S.	20	Aug. 31	Tibia and fibula, middle	Aug. 31	Oct. 6	Some union	Complicated by temporary œdema and erysipelas; good union two months after.
3	Chas. A.	26	Aug. 19	Olecranon process	Aug. 19	Sept. 15	Good union	Some stiffness at elbow which disappeared afterwards.
4	Mary N.	35	Apr. 16	Tibia and fibula, lower third	April 27	.....	" "	Old fracture with necrosis.
5	David O.	40	Jan. 29	Tibia and fibula, middle	Jan. 29	Sept. 22	" "	Emphysematous and great extravasation of blood.
6	Jno. R.	40	Sept. 26	Pott's	Sept. 26	Dec. 20	" "	Fractured by direct violence.
7	Peter G.	39	Nov. 17	Tibia and fibula, upper third	Nov. 18	Jan. 20	Tolerable union	See detailed record. Fractured by direct violence.
8	Wm. W.	15	July 2	Tibia and fibula, lower third	July 2	Oct. 7	Good union	" " " " "
9	Wm. F.	12	June 22	Radius and ulna, middle	June 22	Aug. 5	" "	Fractured by direct violence—struck by a piece of wood; wound small.
10	David W.	28	May 11	Tibia and fibula, middle	May 11	Sept. 15	" "	Tibia protruded; sharp hemorrhage. See record.
11	Albert S.	10	Mar. 12	" " "	Mar. 12	Apr. 24	" "	Fractured by direct violence; tibia protruded. See record.
12	Jno. B.	29	Mar. 27	Tibia and fibula, lower third	Mar. 27	June 15	" "	Fractured by direct violence; piece of fibula removed. See record.
13	Owen K	37	April 21	Tibia and fibula, middle	April 21	Aug. 1	" "	Large abscess formed. See record.
14	Dan. R.	63	Nov. 3	Femur, middle	Nov. 7	Jan. 8	" "	Tibia and fibula on same side also broken. See record.
15	Wm. L.	28	Oct. 22	Tibia, lower third	Nov. 3	Dec. 8	" "	
16	M. R.	16	May 11	Tibia and fibula, middle	May 12	Jan. 17	" "	Femur on same side also broken.
17	Ann H.	28	Mar. 2	Femur, middle	Mar. 3	May 14	" "	Fractured by direct violence; œdema of labium. See record.
18	Jas. H.	45	Jan. 13	Femur, middle	Jan. 13	Mar. 1	" "	Fractured by direct violence. See record.
19	Mat. H.	32	Jan. 24	Tibia and fibula, middle	Jan. 24	Mar. 28	" "	Fractured by indirect violence; small opening. Emphysema and extravasation.
20	Jas. A.	22	Feb. 3	Humerus, middle	Feb. 3	Mar. 9	" "	Small wound; abundant venous hemorrhage.
21	Henry M.	21	Aug. 11	Radius and ulna, lower third	Aug. 11	Sept. 12	Imperfect union	Patient a sickly man; wound small.
22	Jno. R.	30	Feb. 17	" " "	Feb. 17	.....	" "	Had necrosis.
23	David H.	30	Jan. 28	Tibia and fibula, lower third	Jan. 28	.....	Good union	Tissues emphysematous and distended with blood.
24	Wm. L.	28	Oct. 22	Tibia, lower third	Oct. 22	Nov. 30	" "	Struck by locomotive; wound small. Bad scalp wound also.
25	Jas. M.	9	Dec. 7	Tibia and fibula	Dec. 7	.....	Necrosis but good union	Fractured by direct violence. See record.
26	Jas. McD.	37	Oct. 30	Tibia and fibula, lower third	Nov. 5	Jan. 16	Good union	Fractured by direct violence. See record.

We have 26 cases in all, 12 of tibia and fibula, 5 of tibia alone, 1 of humerus, 3 of radius, 1 of ulna, 1 Pott's fracture, and 3 of the femur. The average time of union, so far as given, is 63 days. 20 occurred from direct violence. 23 were put up at once, or within 24 hours after receipt of injury. Of course a great many more cases of compound fractures are recorded in which the plaster splint was used, but as these died from the shock of the injury, pyæmia, septicæmia, or other cause plainly not dependent in any way upon the method of dressing the fractured limb, I have not introduced them here.[1]

Some of these cases were so complicated and otherwise interesting that I give herewith a brief synopsis of the history.

Case VII.—Peter G.; this patient received a simple fracture of the tibia and fibula Nov. 17, from being run over by a light wagon. Contusion not very marked; fracture nearly transverse; gypsum splint applied next day, and patient allowed to go about on crutches. Splint cut down before it hardened, and reapplied with dry bandage. Patient was comfortable until Nov. 28, when he began to feel dull, heavy pain over site of fracture, and drew my attention to a discoloured spot opposite that point, outside of the splint. Removing the splint, I discovered that a small abscess had formed and burst—the abscess communicating with the fracture. Small pieces of bone subsequently came out. A fenestrated splint was applied, and he made a good recovery.

This case goes to prove the porosity of the splint, and its property of transmitting intelligence of the kind cited, viz.: The bursting of a small abscess, not accompanied by sufficient inflammatory or other constitutional symptoms to call for complete inspection of the limb prior to its bursting.

Case VIII.—Wm. W., age 15. Patient fell four stories, striking on his feet. When admitted, he was found to have a fracture of the right external malleolus, opening into the joint. There was also a fracture of the internal malleolus. There was a small wound over external malleolus communicating with the point of fracture. The leg was put up, within four hours after the accident, in a fenestrated splint, the front and sides being left exposed. Necrosis followed, and eighteen small pieces of bone were taken out at intervals. This patient reported to me seven months after the injury, and could then walk without a cane; the wound had completely healed, and there was some motion at the ankle-joint. Owing to loss of substance, the foot is canted to the inner side, so that he is obliged to wear a compensatory sole. Amputation was seriously discussed in his case. His pulse never rose beyond 112, though there was at times considerable pain. After being discharged from the hospital, while yet on crutches and coming back to be dressed, a plaster bandage was put on

[1] Of the 26 cases just given, 21 were treated in wards to which the author was attached, and the measurements in many of these cases were confirmed by other surgeons, and particularly by those opposed to this treatment. The fact that the worst results are in cases which were kept in bed may indicate that the weight of the splint keeps up extension when the patient is on crutches.

by the figure of 8 turn afresh every dressing, as there was a strong tendency of the foot to luxate inwards.

This may serve to disabuse the minds of some of the idea that the application of a gypsum apparatus necessarily takes a great deal of time, for here was a case in which it was applied afresh two or three times a week, as the readiest way of dressing the limb.

CASE X.—David W., age 28. On May 11 patient fell down a hatchway, and a case of gunstocks, weighing about two hundred pounds, fell after him, striking his left leg, producing a compound fracture of the tibia and fibula, near the middle. The sharp upper fragment of the tibia protruded through a wound in front about half an inch long. There was very troublesome venous hemorrhage, and the wound pulsated violently. Patient still suffering from shock, the splint was applied at once and a large fenestra cut. Wound treated with Lister's antiseptic dressing; injected with strong acid carb., and pressure applied. This checked the hemorrhage after about twenty-four hours.

13*th*. Still strong pulsation all around the wound. Firmly bound down with oakum to support.

14*th*. Pulsation gone. Lac plaster excoriates, and is left off.

June 1. Doing very well; very little pain and good appetite.

3*d*. Considerable pain; pulse 120; skin hot.

5*th*. Sharp chill; ordered quinia.

29*th*. Has had no more chills; wound nearly closed.

This patient subsequently had necrosis, and the dead bone becoming loose before the union of the surrounding bone was strong, I removed it through the fenestra in the splint. Patient was discharged cured in about six months from accident.

This case illustrates what may be done with the gypsum splint, with careful watching of the state of the circulation. I confess to having felt some fear, and to having been strongly tempted to remove the splint and place the leg in a fracture-box when I saw the whole surface exposed in the fenestra throbbing strongly. I pinned my faith, however, to the circulation in the toes and the absence of grave constitutional symptoms. This case also illustrates how an operation for necrosis may be performed through a fenestra in a splint earlier than otherwise, as previously alluded to.

CASE XII.—Jno. B., age 27; healthy labourer. On March 27 his leg was caught between two heavy stones. Sharp hemorrhage; fracture compound and comminuted; great contusion and swelling; opening on each side, opposite fracture, both leading down to bone, and from the external one is sharp hemorrhage; piece of fibula, one inch long, removed from external wound; fracture of tibia and fibula low down; patient's general condition good; pulse 90; plaster splint (interrupted variety) applied within two hours from date of injury; Lister's antiseptic dressing used. The pulse in this case did not rise above 104, nor the temperature above 100°. He took no anodynes, and always said he felt comfortable. There was very moderate discharge, not averaging ʒj a day. He was kept in bed about five weeks, and was discharged cured, August 7, able to walk quite well.

This case is noticeable for the absence of any constitutional symptoms whatever, and yet it was a case that many good surgeons would have condemned to amputation.

Case XIII.—Owen K., age 26, was run over April 21, by wheel of loaded coal-cart. Free hemorrhage; fracture of tibia and fibula at middle; probe passes between fragments, and touches tibia at point of fracture; wound about one inch long over fracture of fibula. Put up within twelve hours in "interrupted" splint. Lister's dressing used.

May 1. No marked constitutional symptoms; pulse less than 115 and temperature less than 101° since last note; leg dressed every two days; discharge very moderate.

June 1. New splint, not "interrupted," but simple fenestrated; patient allowed to go on crutches; superficial abscess has developed near wound. Complete cure in five months.

The "interrupted" splint was used here at first because of the contused condition of the soft parts on the inner side of the leg, which it was desirable to have under observation.

Case XIV.—Daniel R. Patient was run over on Nov. 3, by a loaded truck, the fore wheel passing over his thigh, and the hind wheel over the leg of same side. He is found to have a compound fracture of the right femur, near its middle—the wound being entirely over the seat of fracture. Wound small; probe touches bone. There is also simple fracture of right tibia and fibula, near the middle. The leg was put up in a gypsum splint, extending nearly to the wound, the leg being slightly flexed upon the thigh; after this had hardened thoroughly, it was found that traction upon it caused no pain of account, and the thigh was enveloped in a continuation of the splint—traction being made from the lower part. The wound discharged freely for some days a dirty and offensive pus, but it closed after three weeks, and January 8 the splint was removed, and good union of the tibia and fibula found, with good position. The union of the femur was also good; but there was some anterior deformity caused by the upper fragment. The length I could not get by comparison, as there was an old unreduced luxation of the head of the femur of the same side upon the "dorsum ilii."

This case shows that in fracture of the femur, complicated with a fracture lower down, the surgeon need not necessarily give up extension—for this man would allow all the extension that two men could make by pulling upon the splint below—the traction seeming to be mainly received by the anterior and lower surface of the thigh. This probably caused the anterior deformity of the upper fragment, which, in a similar case, I should take special measures to prevent. That the traction did no harm to the fracture of the leg, is shown by the result. The principle is, of course, applicable to any number of fractures not *too* close together, viz., first extending the splint over the most distal one, and then using the continuity thus given in manipulation for the reduction of the next.

Case XVII.—Ann H. was struck March 2, on right thigh, by a piece of machinery which flew from a revolving wheel. She says the thigh bent so that the right knee lay upon the middle of the sound thigh. I found a

fracture at the middle, with shortening 1¼ inch, and great swelling. There was a small wound on outside of thigh, through which a probe touched bone.

March 3. Plaster splint applied; large fenestra cut over wound.

16*th*. Patient trying to get out of bed alone, fell, and broke the splint, which was then removed. The so-called "Bucks extension" was then applied for two days. Patient had no constitutional symptoms whatever. The discharge from the wound has been trifling.

18*th*. Splint reapplied.

19*th*. Considerable œdema of left labium, and some scalding in micturition.

21*st*. Œdema gone; patient sits up all day, but is awkward in using crutches. Discharged cured May 22, with ⅛ inch shortening.

The œdema in this case was the first I had seen, and it resulted apparently from the pressure of the upright bar during the application of the splint, as it was in the labium of the opposite side, and not where it could be explained by pressure of any part of the apparatus.

Case XVIII.—James H. Compound fracture of right femur, just above the middle. He was caught between a coal-box and a coal-cart, which was backing up to it, and crushed. There was a large opening over the apex of Scarpa's triangle, admitting two fingers down to the broken bone; strong tendency of upper fragment to project anteriorly; lower fragment was brought into line by raising the knee and making extension; gypsum splint put on in this position, while patient was relaxed by shock of accident; large fenestra cut over wound and leg swung from cradle. This patient made a speedy recovery, without having had ℥j of discharge from the wound, and with a pulse never above 96 and temperature never above 100°. Result, good union, with ¾ inch shortening. The wound was dressed simply by oakum, bound on.

This case is, in some respects, the most remarkable of all, from the crushing nature of the injury, the extent of the wound, the freedom from constitutional symptoms, and the union of the wound almost by first-intention.

Case XXIII.—David H. Patient's leg was run over by wheel of truck. Fracture of tibia and fibula at junction of middle and lower thirds; bone protruded; much venous hemorrhage; opening small; tissues emphysematous and distended with blood up to knee. Was put up at once in "interrupted" splint. Pulse never above 96; temperature not over 102°. Patient out of bed in three weeks. Good union.

This is another case of absence of constitutional symptoms. In this case, after the splint had hardened, there was some deformity, which was remedied by cutting the connecting band, rectifying the position, and applying a new connecting piece over the old one, showing that in a case of this kind it is not necessary to reapply the apparatus from the beginning.

Case XXV.—James M., age 9. December 7, a heavy iron fly-wheel fell on both legs, producing a compound, comminuted fracture of the right tibia, with a wound about two inches square, over and communicating with

it; also a simple fracture of the left tibia, with an immense lacerated wound on the outer and anterior surface of the left leg. This fracture became compound a week afterward. Both legs were put up at once in the "interrupted" variety of the gypsum apparatus. He had tolerably severe constitutional symptoms once, resulting from the suppuration of a large extravasation of blood which was on the outside of the right leg, and was opened. He had necrosis of the right tibia, but subsequently made a complete recovery in both legs, and was able to walk and run perfectly.

Case XXVI.—Jas. McD. Patient fell, October 30, and struck on an iron chain, breaking his right tibia and fibula at junction of middle and lower thirds. A large piece of bone, nearly the whole thickness of the tibia and three-quarters of an inch long, was removed from the wound. Tissues boggy—leg shortened one inch—put up Nov. 5 in interrupted splint. He made an excellent recovery.

Both the following cases were under the charge of Dr. J. D. Bryant, late House Surgeon at Bellevue Hospital, and in both the author assisted Dr. B. in putting on the apparatus.

Of the cases given in detail, Nos. 7, 8, 10, 12, 13, 14, 17, and 18 were under the author's personal care, and he can vouch for the correctness of the particulars.

Another and more recent case may be adduced, illustrating the application of this apparatus to a fracture not yet alluded to. The history of this case in brief is:—

Thomas C., age 25, single, plumber by occupation, on November 7 fell from a ladder about forty feet, striking on the right side of his head. On rising, he found that his head fell forward upon his chest, and he had no control over it. He had also great pain in the back of the neck. He received treatment, but got no better and came to Bellevue Hospital, November 28, complaining of this pain and loss of control over his head, with constant fear of moving around. The injury was diagnosed as fracture of the transverse process of one or more cervical vertebræ. Patient was kept in bed, as quiet as possible. December 11, somewhat improved. Can bend his neck slightly forwards and laterally, but uses his hands to raise his head from the pillow. He was then put up by Dr. Cushing in a gypsum apparatus made by placing a wide folded strip of "soaked blanket" down the back of his neck and fastening the head to it by circular turns of a plaster bandage around the forehead. The lower end of the strip was secured to the back by similar turns of bandage around the body and across the shoulders, making a sort of jacket with armholes. This apparatus afforded him entire relief from pain in moving about; and from the constant fear that something would happen if he moved suddenly. The apparatus was removed after about six weeks and the man discharged cured.

In conclusion, we will state, in a few words, what we have endeavoured to prove, and to rehearse the means by which we have endeavoured to prove it. I subjoin them arrayed in a tabular form:—

I. To show that the results are at least equal to those of any other instrument, and in some fractures better. To support this ground I adduce

192 cases of simple fracture (50 being of the femur) and 26 cases of compound fracture.

II. That union is not delayed by this splint, as alleged by some. On this point bear 313 cases, 192 of the gypsum apparatus and 121 of the starch, felt, leather, and pasteboard.

III. That the splint may be with safety applied at once in fractures of all bones. In support of this point are 176 cases in which this splint was applied within forty-eight hours from the date of the accident, of which 69 are simple and 23 compound fractures (gypsum apparatus), 23 are simple fractures (felt splint), and 61 simple fractures treated with gypsum splints, of which I could not get complete details as to results, the patients having been discharged from the hospital wearing the splints, etc., but whose records are given beyond the date when we may be sure that no untoward result could arise dependent upon the early application.

IV. To show that great contusion of soft parts, swelling or extravasation of blood do not, of necessity, contraindicate even its immediate application. Here belong 32 cases of simple fracture and 23 of compound fracture thus described.

NOTE.—The author desires to acknowledge the valuable assistance of different members of the present and late House Staff of Bellevue Hospital in preparing this paper. The names of several of them are indelibly associated in hospital memories with the introduction and progress of the gypsum apparatus, and more than one of the visiting surgeons have expressed the belief that the remarkable success attending the use of this apparatus was dependent quite as much upon the skill and intelligence shown in its application as upon the inherent merits of the apparatus itself.

ART. V.—*On the Impracticability of restoring to its Full Length a Thigh-bone shortened by Fracture, as apparently demonstrated by Experiment.* By H. F. MONTGOMERY, M.D., one of the attending Surgeons to the City Hospital, Rochester, N. Y.

A MUSCULAR man, 28 years of age, came under my care April 1, 1872, who had just fallen from a height of thirty feet. He had fractured the frontal bone at the root of the nose, and had also fractured the left thigh-bone at the junction of the middle and lower thirds. The man survived his injuries four days.

The fractured thigh was treated by adhesive-plaster extension, with two weights over two pulleys, with the foot of the bed raised ten inches higher

than the head. The combined weight was eighteen pounds. Sand-bags were not used, as I consider them useless, if not detrimental. A fractured thigh, extended to its extreme capacity of extension, if unobstructed, moves at the hip-joint more readily than at the place of fracture.

The two pulleys, arranged parallel with each other at the same level, and four inches apart, tend to prevent rotation of the limb.

Short splints of thin board were applied to the thigh (probably unnecessary).

When first seen, the fractured limb was about two inches shortened. The weight of eighteen pounds, continued for four days, produced but slight effect. Had my patient survived, I should have increased my extending force by applying as great a weight as could have been retained by the adhesion of the plaster.

After the death of my patient, I wished to determine whether it were possible, by any practicable power, to extend to its full length the shortened thigh, and to bring its fractured surfaces in contact.

Eleven hours after death, the muscles of the fractured thigh were separated, without cutting across muscular fibre, down to the bone. We found a transverse fracture, with one and one-fourth inches lapping. The muscular development was splendid. The body was fixed by a rope between the legs, fastened above the head to the wall, and another strong rope was placed around the ankle of the factured limb. Continued and repeated extension was made by four men together (applying a force probably equal to a thousand pounds over a single pulley). The limb could be brought down by this means to half an inch lapping of the ends of the bone, but no further. The lateral pressure of the extended muscles was observed to be great, quite sufficient to prevent displacement, if the limb could be brought down to its full length, either in an oblique or transverse fracture.

We then applied to the extending cord, a weight of one hundred and twenty-six pounds over a pulley, and left it upon the limb from four o'clock P. M. to nine o'clock A.M. (seventeen hours). The bones were then found lapped one-fourth of an inch. From the above we may conclude that a transverse fracture of the thigh-bone, with slightly shortened limb, will produce greater deformity than an oblique fracture with the same shortening.

Some months ago I had an opportunity to see and feel the end of the bone in a transverse fracture of the thigh in a living subject, three months after the injury, where there was a shortened limb, where union had taken place, and where the greatest weight that could be retained upon the limb had been constantly applied for seven or eight weeks. It was a case of compound fracture, where the patient eventually recovered with a slightly shortened limb, and with adhesion of the extensor muscles of the leg to the thigh-bone. In this case we found that the portions of bone which

lapped had been absorbed down to the medullary portion, on the side where they were in contact, and had thus formed two flat surfaces which had united.

I venture the opinion, that in most, and probably in all, cases of fracture of the femur, whether transverse or oblique, *with shortening*, the fractured ends of the bone cannot be placed in exact apposition by any force which can be safely applied to living tissue.

It is admitted by most modern writers on surgery, that shortening of the limb in fracture of the thigh is to be expected, and is generally found after proper treatment. Would it not be more correct to say, it is always found (when produced by a fracture), because a shortened thigh-bone *cannot* be fully extended to its normal length after the broken ends of bone have passed each other?

ART. VI.—*Cases of Stricture of the Rectum treated by Different Methods; one of them by Electrolysis.* By WM. R. WHITEHEAD, M.D., of New York, Physician for Diseases of Women at the Northwestern Dispensary.

THE unsatisfactory results which so often follow the ordinary treatment of stricture of the rectum, is a sufficient reason to call attention to the following cases. They illustrate in a comparative manner the value of certain modes of treating this disease, which is confessedly a very distressing one, and exceedingly difficult to manage. It is proper, however, to preface the relation of these cases by the statement, that I believe that the most effectual way to cure fibrous stricture of the rectum is by a thorough division of the stricture posteriorly, and by dilatation afterward by means of elastic pressure, such as I have described in a preceding number of this Journal (Jan. 1871).

CASE I.—Mrs. F., a very intelligent lady about forty-two years of age, consulted me at the suggestion of Dr. Wm. George Thomas, of Wilmington, N. C. For a number of years she experienced much distress and difficulty in relieving her bowels; but was not aware of the presence of a stricture of the rectum until examined by Dr. Thomas about twelve months previous to the time at which I first saw her. He was of the opinion that the stricture was not cancerous, and this opinion was strengthened by a microscopic inspection by Professor Leidy, of Philadelphia, of some of the dejected matters which were sent to him for examination.

On the 15th of May, 1871, after she was thoroughly etherized by Dr. F. H. Deems, I made a careful examination of the rectum and contiguous parts. There was a tight stricture of the rectum more than an inch in length, and which extended from opposite the lower part of the Douglas cul-de-sac upward, and above it. A considerable mass of indurated tissue

formed the stricture anteriorly, and occupied a very extensive relation with the peritoneal covering of the rectum. Mrs. F. informed me that she had for ten or twelve years worn a pessary in the vagina, and that she had once worn this instrument about a year without removing it. She was disposed to attribute the stricture to inflammation caused by the long-continued pressure of the pessary against the rectum, and I am quite inclined to adopt this opinion. The case appeared to be a formidable one for operation, and not being at that time so well convinced of the necessity of a thorough section of the stricture as I am at present, I was undecided whether to cut or rupture it.

However, believing it necessary to resort to either one or the other of these procedures, in order to obtain subsequently a desirable degree of dilatation with the elastic pressure, which I believe to be the safest method of dilatation, and an effectual one for the treatment of most cases of stricture of the rectum, on the 18th of May, assisted by Dr. J. B. Landeta, Dr. Francis H. Deems, and my present clinical assistant, Dr. A. Landeta, I ruptured the stricture with Sims's dilator, and completed the rupture with my index finger, having directed my force principally against the posterior wall of the gut. The rupture was made without difficulty, but I was apprehensive about the extent of this rupture and the danger of laceration of the rectum. This danger I do not believe to be overrated; and since I have quite satisfied myself about the safety with which a deep incision of the rectum may be made posteriorly, I prefer incision to rupture of a stricture except when seated quite near the anus. After the operation a piece of sponge about the size of a small orange and with a string attached was readily passed up the gut beyond the previous seat of stricture, so that the part could be well seen with Sims's speculum by all present. At the time of this operation, I was unaware of the existence a short time before, of a slight subacute peri-uterine inflammation. The knowledge of this condition gave me considerable solicitude about the safety of the patient. She had some peritonitis, which was controlled however with morphia in fractional doses given at intervals. The use of the duck-bill speculum, caused a fissure of the anus, which was cured by rupture of the sphincter ani muscle with the fingers. The peritonitis interfered with the immediate after-treatment by dilatation, but after six or eight days she used very effectively the instrument which I have recommended for elastic dilatation, but which has been so modified as to be much preferable to that first used. Four, five, or six rubber condoms one over the other, and properly adjusted to the instrument, will offer as much resistance as a stout Barnes's dilator, and are much easier to introduce either by the patient or by the surgeon. The probe which is attached to the instrument, and which is inside of the rubber bags, should be made of whalebone, and should be of the requisite degree of flexibility, and covered with a piece of rubber tube about the eighth of an inch in diameter. The tube protects the whalebone from the water, which would soon soften it and make it worthless as a director. There is no difficulty about the use of this instrument in the hands of an intelligent patient when once shown how to adjust properly the condoms to the instrument, and to its directing whalebone staff. A single rubber bag sufficiently stout would appear to be much preferable; but attempts have been made by instrument-makers of this city to make such bags for me for this purpose, but there has always been some defect about them, which made them dilate very unequally, and burst at one of the seams or elsewhere. Rubber condoms or capotes can be obtained anywhere; they

are used by oculists for ice-bags, and, introduced into the natural cavities of the body and distended with ice-water, are useful for stopping hemorrhage. Half a dozen condoms, properly adjusted one within the other, and distended with water by means of a Davidson's syringe attached to a tube with a stopcock, I have found to be a good way to treat vaginismus after section of the vaginal sphincter, or even when cutting of this sphincter is not required.

It is very important to wash out the rectum three or four times a day with carbolized injections. A lacerated wound or cut of the mucous membrane of the rectum, constantly bathed with excrementitial matter and with pus, becomes without this means a great source of danger from septicæmia. During the latter part of July I received from Dr. Thomas a letter from which I make the following interesting extract:—

"The lower part of the stricture is decidedly relieved, the upper part not so completely. She has devised a modification of your capote dilator which struck me with force, and I think will answer an admirable purpose. She puts a silk bag between the rubber capotes, and ties them to the guiding staff. The silk bag may be stretched to its utmost capacity, and is firm, and is well protected from both the injected fluid and the secretions of the rectum by the condoms. It operates well, and upon trial I think you will be pleased with her ingenious improvement. She has used the dilator daily since she left New York. I advised her to use it once every three or four days only, during two to four hours at a time, and to gradually enlarge the silk bag."

The width of the silk bag, a paper model of which Dr. T. sent to me in a letter, is two and a quarter inches. In a letter from the patient dated December 16th, 1871, more than six months after the operation, she stated that she uses the dilator quite frequently, and that her physician recommended her not to use it oftener than once in two or three weeks. She stated that her appetite was excellent, and that she was able to attend actively to her household matters; she had gained flesh, and all of her friends had observed and remarked upon her improved condition. We believe that a rather frequent, but judicious and long-continued, use of the elastic dilator in such cases to be very necessary.

CASE II.—Last summer Dr. A. Worthington, of Ontario, Canada, consulted me by letter relative to Miss M., who had stricture of the rectum of a number of years' standing, and which was complicated with a recto-vaginal fistula. The parents of the lady, by advice of Dr. W., their family physician, requested me to visit her at her residence near the village of Wroxeter in Canada West, and, if necessary, to perform such an operation on the stricture as the necessities of the case might require. Miss M. was thirty-four years of age; I found her anæmic, greatly emaciated, exceedingly inanimate and taciturn in her manner. I was much discouraged by the appearance of her general condition. Menstruation had ceased about two years before she consulted me, but previously it was regular. During two or three years there were discharges of bloody water from her bowels, together with lumps which her mother said resembled fat meat. During a period of four years she had occasionally hemorrhages from her bowels; altogether nine or ten in number. During the last three years her health had gradually declined. July 12th, 1871, the day on which I first saw her, assisted by Drs. Worthington and Byron Crandell, of Clifford, she was etherized and I made a careful examination of the rectum, which revealed

by the vaginal touch a very indurated cervix and a recto-vaginal fistula on a level with the posterior lip of the cervix. The fistula admitted with some constriction the end of the index finger. The uterus was normal in size, but bound down posteriorly by adhesions. The rectal touch discovered two strictures with the recto-vaginal fistula between them. The lower stricture was inconsiderable in size, and was situated immediately above the sphincter ani muscle. The upper and principal stricture was seated three inches higher up the gut. The walls of the rectum between the strictures were firm, rigid, and posteriorly were slightly *nodulated* and suggestive of an interstitial, malignant deposit. The upper stricture admitted the end of the index finger. While the patient was under the influence of ether and in the semi-prone position, I tore the stricture posteriorly with the index finger of the right hand hooked into it. With two fingers of each hand I ruptured the sphincter ani muscle, and at the same time the indurated tissue of the rectal wall just above the sphincter. I then felt a fibrous band running somewhat obliquely across the posterior part of the upper stricture. I cut this band with a small knife which was concealed within, and could be projected at will from, a flattened sheath. The whole stricture immediately yielded, and by means of Sims's speculum, nearly the whole of the lower part of the rectum was plainly brought into view. The cut was made on the median line and posteriorly. There was not a teaspoonful of blood lost during the operation. The danger in this case was from peritonitis and from septicæmia. The first I believed could be controlled by the appropriate use of morphia; the second could be prevented by washing out the rectum several times a day with carbolized injections. During the first twenty-four hours she was inefficiently nursed, and took the morphia very irregularly. She was allowed several times to sit up and strain at stool for one or two hours at a time, after which she was very much exhausted. When I saw her the next day, I urged the parents not to allow her to sit up, but to have a bedpan put under her when required, and to procure immediately, if possible, an experienced nurse. During the second night after the division of the stricture she had an alarming collapse, and vomited once. She was resuscitated by the application of hot bricks to her feet and by teaspoonful doses of brandy and water. I remained at the house with her during the night and part of the following day.

On the 14th of July a good nurse was obtained, and the pulse, which had been at 140, under the influence of a solution of sulphate of morphia regularly given fell to 120, and the patient's condition was better. There was not so much tenderness about the lower part of the abdomen. The mother persisted occasionally in giving the patient her medicine, and instead of a teaspoonful of a solution of morphia which contained the one-eighth of a grain of this substance, gave by mistake a teaspoonful of a saturated solution of carbolic acid. This was used largely diluted with water to wash out the rectum. Dr. Crandell was in the room at the time that this mistake occurred, and Dr. Worthington and myself were in the adjoining room. The whole interior of her mouth was blanched, and she experienced very great distress in swallowing some milk which I requested her to drink immediately. After a few hours the pulse ran very high, and the distressed feeling about her mouth and throat increased, and she died about twelve hours after the swallowing of the carbolic acid, which contained only five per centum of water. I regarded this case as a very unfavourable one, and which possibly under different circumstances I should have declined to operate upon. But it was quite evident that her only chance to obtain any

relief was in a careful division of the stricture or by its rupture in the manner in which I effected it, to be followed by methodical dilatation with the rubber bag distended with water. She had commenced under better nursing rapidly to improve, and would I believe have recovered had she not swallowed the carbolic acid.

CASE III.—The following case is interesting from the fact that several attempts were made to disperse, by electrolysis, the interstitial deposit which formed an indurated mass that caused the stricture of the rectum.

Mrs. K. consulted me at my clinic at the Northwestern Dispensary more than eighteen months ago, for stricture of the rectum. The stricture admitted with difficulty the end of the finger, and extended from opposite the Douglas cul-de-sac, upward above it, and beyond the reach of the finger. The stricture was unmistakably the result of syphilitic ulceration of the rectum, and the patient had a secondary eruption on the skin. This eruption, under appropriate treatment, disappeared, but reappeared once or twice, and she, at present, has an ulceration of the leg of syphilitic appearance. Soon after I first saw her, she was etherized, and the stricture was cut with a curved bistoury anteriorly, and posteriorly at its lower part; but as I could not reach the upper part of the stricture with my finger, the operation was necessarily incomplete. The knife was securely wrapped, except near its point, with cotton, and the cutting part was well protected by the end of the index finger of the right hand. Attempts were irregularly made to dilate the stricture with the rubber bags, and by forcible dilatation with the finger. I held in reserve, however, this case for another cutting operation, and decided that I could safely make a very extensive incision, if required, on the posterior wall of the rectum. My opinion in this matter was strengthened by a careful examination of two or three cadavers with reference to the relations of the peritoneum to the rectum. Besides, Professor Darling, of this city, professor of anatomy in the University Medical College, kindly called my attention to a dried specimen which well illustrated these relations in a manner usually overlooked by anatomists. An incision made posteriorly, at any point along the median line, from just below the promontory of the sacrum, and between the folds of the meso rectum to the sphincter ani, will not wound the peritoneum, or be likely to be followed by hemorrhage. But the peritoneum covers the sides of the rectum quite low down, and the peritoneal duplicature of the utero-sacral ligaments in relation also, anteriorly with the rectum, may, especially in partial prolapsus of the uterus, draw the peritoneum unusually low down on the sides of the rectum.

I was attracted to the use of electrolysis in the treatment of strictures of the urethra by Tripier's article in the *Arch. Gén. de Médecine*,[1] and besides, I was invited by Dr. Robt. Neuman, of this city, to see with him a patient with stricture of the rectum, and to which he made one application of electricity with Stöhrer's battery of sixteen elements, and subsequently used my dilator for elastic pressure. I am not quite sure which electrode Dr. Neuman applied to the stricture. As an immediate result of this application the stricture was considerably dilated. Quite recently, a post-mortem examination of the case afforded Dr. Neuman an opportunity to exhibit to the New York Pathological Society the rectum of this patient. It seems that, having obtained some relief, she became careless

[1] La Galvano-caustique Chimique, par le Doct. A. Tripier, Arch. Gén. de Méd. 1866, vol. i. p. 18.

and neglected herself; finally she got worse, and had fecal fistulæ to form. The specimen shown to the Society interested me exceedingly, but I was unable to draw any definite conclusions from the appearance of this specimen, as to the value of electrolysis in the treatment of strictures of the rectum. However, I had, after having seen Dr. Neuman's case, obtained Dr. Wm. B. Neftel's co-operation in the electrolytic treatment of the stricture of Mrs. K., the subject of the third case, and which I am now reporting. The electrolytic treatment of this case extended over a period of one month, and during this time four applications of electricity were made. All other kinds of treatment were purposely avoided, so that the electrolytic action might be well tested. The patient was not etherized, and the first application was made with a new galvanic battery of Curt Mayer, of New York (a platina zinc battery). A cathode made for the purpose, consisting of hard rubber and a solid brass cylinder rounded at the end, was introduced by me partially into the stricture. Dr. Neftel commenced with one element, and increased gradually, one by one, to twenty elements. There was a slight and gradual increase of pain. A current of twenty elements was maintained for fifteen minutes. I repeatedly examined the part during this time. The applications lasted about half an hour. There was an immediate dilatation of the lower part of the stricture only.

On the 16th of January, 1872, a week after the first application, another one was made with Remak's battery. Dr. Neftel thought that it would be well to isolate with shell-lac, or hard rubber, the rounded end of the brass cathode. It was isolated at its end with shell-lac, and introduced by me into the stricture. The anode, in the shape of a flat disk, and separated from the skin by a piece of wet cloth, was, as in the preceding application, applied to the hip, and shifted about from place to place, when the heat developed at the anode, or positive pole, became unendurable. The application began with two or three elements, and was gradually increased to fifteen, at which number the current was maintained for five minutes, with a deflection of the needle of the galvanoscope of fifteen degrees. The current was then gradually, by the addition of one element at the time, increased to twenty-one elements, as denoted by the position of the winch in the current-selector. A current of twenty-one elements was used during five minutes; it was then interrupted, and I examined with my finger, and found that the upper part of the stricture was considerably dilated. The circuit was again closed, and the cathode reapplied to the central part of the stricture. The applications this time lasted altogether fifteen minutes, and the patient afterward did not feel uncomfortable. Possibly it may be well to observe that Dr. Neftel used the elements of Siemens, which were substituted for Remak's elements, and were contained in the box of Remak's apparatus. It appears to me that, for accuracy of observation, it would be desirable to adhere to the use of one apparatus.

January 24, a third application was made. Previously the patient stated that her bowels were more easily moved, and an examination revealed that the stricture was more dilated. A brass cathode, isolated at its end with hard rubber, was introduced into the stricture, and the anode applied to the external part of the thigh. Twenty-three elements of Siemens were used during twelve minutes, with a deflection of twenty-three degrees of the needle of the galvanoscope. Twenty-one elements were used for eight minutes longer. A considerable quantity of frothy mucus,

caused by the evolution of hydrogen at the cathode, issued from the rectum. The applications this time lasted altogether twenty minutes. After the discontinuance of the current I found that the upper part of the stricture was much dilated, but that the middle part was still constricted. A week later, another application of electricity was made much in the same manner as this one, but of which I failed to keep any notes. I decided not to use dilatation after these applications, but to permit a few months to elapse before I examined her. I believe this was also Dr. Neftel's suggestion.

April 16, 1872, she came to the Northwestern Dispensary, and was carefully examined by my clinical assistant, Dr. A. Landeta. The stricture with difficulty admitted the end of the index finger, and was quite as tight as before the electrolytic treatment.

Possibly it may not be out of place, in connection with this case, to attempt to define what is meant by electrolysis as applicable to the dispersion of certain tumours, or of the induration of the tissues, as in stricture. The different effects of the two electrodes on the living tissues should be distinguished one from the other. These effects are modified by the use of batteries of great electro-motor power, or of feeble power, or composed of many small elements, but of great tension. It seems to me that a multitude of different circumstances influence the effects of the currents. The kind of electrode used; such as needles, blunt or flat electrodes, and these last with or without moisture, modify the effect. The moving of the electrodes, and, if needles be used, the position in which they are stuck into the flesh, whether parallel or in a line continuous with each other, are also worthy of attention. The effects must necessarily differ widely, because, if, as in the last case, the needles intersect, or are continuous with each other, especially should their points be near together, we may have a bad slough of the tissues in the place of a resolutive or electrolytic action, such as is supposed to occur at the cathode of a galvanic current, under conditions favourable to the development of the peculiar effects of the negative current at this pole. It is not at all my object to discuss the subject of electrolysis, but I, probably in common with many others, would like to know what is its practical value, and also something about the right indications for its use, and the proper manner to develop the electrolytic action.

It is necessary, it seems, to distinguish well the decomposing power of the current from its heat-producing power. The chemical and decomposing power, or electrolytic effect, is supposed to take place at the cathode, and is caustic in its action, but caustic after the manner of alkaline caustics. The heat-producing power of the current is developed at the anode, at which may be produced a disorganization of the tissues immediately around it, similar to that caused by the actual cautery. If the anode be a thickly plated gilt needle, thrust into the tissues, it will be corroded by oxidation, and there may be considerable difficulty in withdrawing it. The disorganization of the tissues immediately around the needle will be

proportional to the greater or less intensity of the current. If the cathode, or negative electrode, be also a needle, and thrust into the flesh, no corrosion of this needle occurs, but soon the tissues around it will become emphysematous by the escape of hydrogen. An alkali is evolved at this electrode, and an acid at the other. It would seem that there occurs a mechanical disassociation of the tissues by the action of the escaping hydrogen, and a dissolving and fluidifying effect caused by the alkali liberated at the cathode. If the anode be a plate or disk, the heat may be so great as to burn the patient severely. It should be recollected that the positive electrode is at the end of the wire which leads from the negative metal, and the negative electrode at the end of the wire which leads from the positive metal, and which would be the zinc, if this be used, in a zinc and copper battery.

The direction of the positive current is from the metal oxydized to that which is not, and the direction of the negative current is the opposite. Tripier's experiments on strictures of the urethra gave immediate results which were seemingly satisfactory. The strictures in each case after the application of the cathode were enlarged, but their condition a few months afterward is not stated. He concludes that the negative galvano-cauterization, or that obtained with the cathode, is an excellent substitute in certain cases, for the caustic of Filhos, and that the cauterization obtained with the anode may replace that of the strong acids, or the actual cautery. He claims that the cicatricial tissue which results from cauterization with the cathode is soft, thin, and not resilient, and that the inodular tissue produced at the anode has the usual character of such tissue. The greater the electro-motor force of the battery, the more rapid, and the more painful will be the cauterization. Whichever electrode is selected for cauterization, the effect would be, apparently, to burn a larger hole through the stricture, and it seems to me that dilatation should be used afterward in order to obtain any benefit from this mode of treatment.

CASE IV.—As an example of stricture of the anus and lower part of the rectum, caused by the injudicious application of caustics for the cure of hemorrhoids, the following case may prove interesting.

Through the recommendation of Dr. Horace P. Farnham, of this city, a gentleman from one of the Western States recently consulted me for an intolerable itching about the anus, and for great difficulty in evacuating his bowels. There were hard and slightly elevated folds of the integument immediately around the anus, and on introducing the finger into the rectum, the tissues about the anus felt very much indurated, and formed a tight stricture at the anal orifice. On remarking to him that probably some caustic application had been made to the parts, he replied that a few years ago he was treated for hemorrhoids by several applications of caustic, and that he had since noticed a gradually increasing difficulty in relieving his bowels. I told him that gradual dilatation was, in his case, the only safe and effectual means of relieving him, that division of the sphincter ani, at present at least, was unnecessary, and that dilatation

would also afford him relief of the itching. I advised him to have a number of smooth and slightly conical plugs, about two inches long, made of maple wood, each with a handle, and also with a flange, to prevent it from slipping too far into the rectum. These plugs should be graduated in size, from that of the finger to that of the largest bougie.

ART. VII.—*Fibrous Tumour of the Base of the Tongue.* By J. F. FITTS, M.D., of Francestown, N. H.

DR. J. H. POOLEY, of Yonkers, N. Y., reported in the April, 1872, number of this Journal (p. 386) a case very similar to one which came under my own observation, and such cases being rare, I offer the following report of it. Dr. Pooley speaks of the tumour in his case as unique, especially as regards situation—the tumour in my case was in this respect precisely similar.

Miss H., aged 30, sent for me about the first of May, 1869, for some slight affection of the throat, the result of a sudden cold. In examining the throat while depressing the tongue, I noticed a tumour rising up at its base, of about the size of a filbert. The patient was entirely unaware of its existence, and it might have remained there unnoticed for months but for this chance examination. There had been no pain, soreness, dysphagia, dyspnœa, or hemorrhage to call attention to any lesion of the throat or tongue. In fact, she had never had any trouble with her throat, except such as resulted from taking cold. The tumour was situated exactly in the median line, on the base of the tongue, and so far back that it was somewhat difficult to pass the finger behind it. It was exceedingly hard, incompressible and immovable. It was sessile, or at least, if there was any pedicle, it was so broad that it was not easily made out. Examination caused some pain but not at all severe. The case was under my care until the following November, when by my advice she went to the Massachusetts General Hospital for a thorough laryngoscopic examination, and operation if deemed advisable. In the mean time, the tumour had steadily increased in size, until on opening the mouth and protruding the tongue, and without using any depressor, it filled almost the whole of the fauces. It had caused but very little trouble, perhaps no more than might have been produced if the imagination were only worked upon.

I accompanied her to Boston, but as she could not then be admitted into the hospital, we returned home without anything being done, save a superficial examination by the resident physician and one surgeon, but no opinion was advanced. Subsequently, in January, 1870, she returned to the hospital, was examined and operated on by Prof. Bigelow. The tumour was removed *en masse*, with the écraseur, the patient being under the influence of ether. At the time of its removal, the tumour was the size of a pigeon's egg.

I was not present at the operation, but I learned that there was no alarming hemorrhage, and the patient made a good recovery; was at home in the course of three or four weeks, and able to attend to household duties.

The tumour was pronounced fibrous, possibly fibro-recurrent, but to this date there has been no return. No treatment with a view of reducing the size of the growth was attempted, save for a short time the syr. ferri iodidi was administered, but with no perceptible effect.

On taking charge of this case, I was considerably puzzled, as, on consulting the standard authorities, I could find very little in regard to tumours of the tongue, and *that little very unsatisfactory.* Two or three of the physicians in the neighbourhood had never seen anything like it, and several of my acquaintance to whom I mentioned the subject had never observed such a case. Under the circumstances I was led to believe that a tumour in this situation was very rare. Indeed, I found only two allusions to the subject of tumours and growths of the tongue. One was a case reported in the *Boston Med. and Surg. Journal*, the date of which I cannot recall, but possibly the same which Dr. Pooley has transcribed into his paper. The other, more satisfactory, was by George Duncan Gibb, "Diseases of the Throat and Windpipe," second edition, p. 342, London, 1864. Under the head of "Tumours and Growths" of the tongue, Dr. Gibb says:—

"In most of our modern works there is scarcely even a mention made of tumours originating in the tongue, yet cases are presented to our notice every now and then. In the museum of the College of Surgeons is a fatty tumour (No. 190), removed from the substance of the organ. Mr. Paget removed an oval bilobed tumour from the tongue of a young man, the size of a small marble; it was near the apex, and had been growing for three years.

"In July, 1862, I removed a fibrous tumour the size of a small marble, with the aid of the laryngeal mirror, from the left side of the back of the tongue of a married lady, which caused great inconvenience and suffocative attacks, from its coming in contact with the left side of the epiglottis."

This last case more nearly resembles the one reported by Dr. Pooley and my own, than any of which I have seen a description. Except in *exact* situation and size, they would seem to be very nearly identical, only my case had no characteristic symptoms, and Dr. Pooley's was marked by severe hemorrhages.

Dr. Gibb then gives an account (with an illustration) of a case of congenital tumours of the tongue, three in number.

"The smallest was the size of a pea; the next as large as a shilling, and the largest two and a quarter inches in length. Each tumour was attached by a narrow pedicle. When in *situ*, the anterior tumour lay upon its dorsum; speech was scarcely impaired, and there was no difficulty in mastication."

The largest one was removed and proved to be fibro-cellular in character.

The case I have reported seems to be precisely similar to that reported by Dr. Pooley, except in size, and the fact that his was accompanied by distressing symptoms and alarming hemorrhage. I think, however, that had an operation been delayed sufficiently long, the same symptoms would have been developed in my case.

ART. VIII.—*Carbuncle over the Cervical Vertebræ, in a Patient eighty-two years of age, successfully treated.* By JOS. B. WHITEHEAD, M.D., of Norfolk, Virginia.

IN the *Lancet* (December 11, 1869, page 800), Dr. Peter Eade, of the Norfolk and Norwich Hospital, relates a case of cervical carbuncle successfully treated by carbolic oil. "The patient was 74 years old, and a man remarkably hale, stout, and robust for his age, free from other bodily disease." As the patient, whose case I am about to relate, is eight years older, of feeble health, and had cutaneous erysipelas, I have deemed it not unworthy of notice.

Miss J. C. A., a lady aged 82, having suffered with cutaneous erysipelas of the face, head, right ear, and left arm, for about three weeks, was finally induced to send for me, March 15th, to examine what she supposed to be a large and painful boil. I found her much prostrated, and suffering very great pain. The erysipelatous inflammation had not disappeared, but was subsiding. What she supposed to be a boil was a carbuncle five inches in diameter, situated to the left of the lower cervical vertebræ, and well characterized by the usual induration, by its dusky red inflamed integument, and broad flat surface, with five or six honey-comb openings in the centre. I at once decided upon crucial incisions to relieve the extreme tension and accompanying pain, but on making the first cut, about half an inch in length, she became so much prostrated that I desisted and ordered whiskey as a stimulant. Applied flaxseed-meal and laudanum poultice, and directed that she should take tinct. ferri chloridi et potassæ chlorat. every three hours, and tinct. cinchonæ comp. before breakfast and dinner, and liquid nourishing food.

16*th and* 17*th.* Patient positively refused to have anything done to the carbuncle, except to allow the poultice to be applied, which I had changed to yeast thickened with bread crumbs.

18*th.* The area of inflammation is extending upwards, and a large slough threatened. As she refused to have it incised, I persuaded her to allow me to treat it by pressure with strips of adhesive plaster applied concentrically, as recommended by Dr. John Ashhurst, Jr., in Erichsen's Surgery. Before completing this procedure, however, she consented to have it incised, remarking that she would rather be cut than strapped so tightly. Having stimulated her with whiskey, I made a deep crucial incision into it, about an inch in length, and applied the poultice. Ordered her medicine to be continued and porter *ad libitum.*

20*th.* General condition improved; states that she had suffered no pain since the night after the incision was made. In the centre of the carbuncle, there was a hole about half an inch deep, and about three-quarters of an inch in its long diameter, which contained dirty pus. Its edges were covered with slough, and stringy connective tissue, and for at least half an inch all around there was slough beneath the integument. I cauterized the edges and also beneath the integument, freely with the solid nitrate of silver, and would have stuffed the hole with lint saturated in pure carbolic acid (as recommended by Dr. J. C. Nott, of New York, see *American Journal of the Medical Sciences*, page 596, April, 1871); but she resisted so stoutly that

I simply syringed it with a solution of carbolic acid, gr. x, and water ℥j, and applied ceratum resinæ comp., which was suggested to me by the encomiums bestowed upon turpentine as a local application in gangrenous sores and carbuncles by Drs. Bartholow and Thielman, as stated in Stillé's Therapeutics.

22*d*. Area of inflammation diminished; its surface but slightly raised, and about one-eighth of an inch from the central aperture (which had enlarged slightly) a healthy line of demarcation is established. The erysipelas has disappeared, and she is able to walk about her room, and is gaining strength. Continued the same local treatment, but substituted as a tonic, tinct. cinchonæ comp. with acid. sulph. aromat.

23*d*. Sloughy connective tissue has not separated; repeated injection and left lint saturated in the acid, in the hole.

24*th*. Used the injection and applied lint as before, but now applied an ointment of carbolic acid gr. xvj, benzoated lard ℥j, and continued its use.

26*th*. A most remarkable change has taken place in two days—the whole wound (which is now about the size and shape of a small almond) is covered with healthy granulations, its edges I have drawn together with a narrow strip of adhesive plaster, and it will heal in a very short time, leaving a cicatrix, which a much younger lady would give herself but little trouble to hide, even in arraying herself in ball costume.

In reviewing this case, I am decidedly of opinion, that the deep crucial incision was of the greatest benefit, and was required, notwithstanding her extreme old age. The ceratum resinæ comp. was of some benefit, but did not check the sloughing.

As regards the use of alcohol, I am satisfied that my patient would have died had she not been stimulated freely with ardent spirits, wine, or malt liquor.

To the incisions and carbolic acid I attribute the cure of the carbuncle, and when an opportunity presents itself, I shall try Dr. Nott's plan of using pure carbolic acid.

No. 156 MAIN STREET, March, 1872.

ART. IX.—*Accidental Irideremia, or Complete Removal of the Iris by the Finger-nail of an Antagonist.* By JULIAN J. CHISOLM, M.D., Professor of Operative Surgery in University of Maryland; Surgeon in Charge of the Baltimore Eye and Ear Institute.

J. M., aged 37, presented himself for treatment at the Baltimore Eye and Ear Institute, suffering from chronic inflammation of the left cornea, with partial loss of transparency. My attention was attracted by the striking contrast which his two eyes exhibited. The left eye, for which he desired treatment, was of a bright blue colour, whilst the entire corneal portion of the right eye was black in colour, with the exception of a white triangular scar 1½ lines wide at its base, which rested upon the lower edge of the cornea with apex extending vertically a little beyond the pupillary region.

To a casual observer, the right eye presented the appearance of an old and severe injury with anterior chamber filled with blood blackened by long exposure; or of an eye under the fullest effects of strong atropia instillation, except that there was no iritic ring bordering the enlarged pupil. Supposing that the eye was useless, what was my surprise, in throwing the light from an ophthalmoscopic mirror upon this black eye, to find it brighten with the true red reflex of a healthy choroidal fundus. The further use of the objective lens indicated transparent media and a normal disk. Upon inquiry, this black unpromising eye proved to be his good one, whilst the normally blue one had its usefulness much marred by the haziness of the cornea.

The patient furnished the following history of himself. Formerly his eyes were both blue and strong, until three years since, when he came out of a fight with one eye black as it appears at present. During the fight his antagonist had gouged him in the right eye. Under excitement he suffered no pain, and was not aware at the time that his eye had been injured. The next day he discovered the change of colour of his eye, and detected a shred of membrane protruding from a scratch in the front of the eyeball. After one or two days this fragment, which was hanging out, came away, leaving a white scar over the sight. No severe inflammation followed upon the injury, and so little inconvenience was experienced that he did not think it necessary to seek professional advice, nor did he lose a day's work. Cold water-dressing was the domestic remedy applied. Until the left eye became inflamed one month since, both eyes had been in constant use, and were believed to be equally good.

A careful ophthalmoscopic examination revealed no trace of iris. When the thrust of the finger had driven the nail through the cornea, it had in some inexplicable manner entered the pupilary opening, and passing between the capsule of the lens and the iris, had thrust this latter membrane against the inner face of the cornea. Acting then as a hook the finger-nail had drawn the iris out of the corneal wound, detaching it from the ciliary ligament, the edge of adhesion being weaker than the cohesion of iritic substance. The final escape of the protruding iris had been effected by the continued drawing action of the ever-winking lids. The finger-nail had in no way injured the lens, nor the accommodation of the eye. Although the corneal cicatrix contained between two descending radii involved about one-tenth the surface of the cornea, light could freely pass around this obstruction and make a perfect image upon the central axis of the retinal chamber. The eye was myopic. I had no means of knowing whether it had not always been so. With this eye he could read No. 1 Jæger's Test Types at 4 inches, and No. 20 at 8 feet. With the optometer and a ten inch convex lens his range of accommodation for "Brilliant" type was near point three inches, far point five and a half inches.

Among the many miraculous escapes from accident, this very interesting case stands alone, and it is nearly impossible to conceive how it can repeat itself.

ART. X.—*Cases of Ovariotomy.* By WASHINGTON L. ATLEE, M.D., of Philadelphia. (Reported by J. EWING MEARS, M.D., of Phila.)

CASE 232. *Multilocular Ovarian Tumour; Extensive Parietal, Omental, and Vesical Adhesions; Incision six Inches in Length; Recovery.*—November 22, 1870, Miss J. M., of New Alexandria, Westmoreland County, Pennsylvania, consulted Dr. Atlee by the advice of her physician, Dr. John Semple. She was 25 years old, and first menstruated at the age of fifteen. For the first few years the menses were irregular, occurring at intervals of two and three months. They were painful. Afterwards they became more regular, and continued so until September, 1869, when they recurred every other week, gradually diminishing in quantity, until February, 1870, after which they were entirely suspended. There had been no difficulty in urination, and the bowels were regular.

In March, 1868, she received a very severe injury which confined her to bed for a week with very severe pain in the right side and back. This was followed by intense pain in the right side for a week before each menstrual period, causing her to go to bed.

About March, 1869, she noticed a swelling at the umbilicus and a tumour to the left of it. She consulted Dr. Semple, who tapped her six times from the 7th of June, 1870, to the 9th of November, 1870, removing a bucketful of fluid each time. The fluids were transparent, the first of a dark-green colour, the last straw-coloured. Tapping did not greatly diminish the size of the abdomen. Her former weight was 170 pounds.

Emaciation was marked; the abdomen much larger than a woman's at full period of gestation; symmetrical in shape. Fluctuation indistinct and localized—the mass evidently consisting of many cysts. Pelvis free, uterus central, and the os and cervix small. The sound would not enter. The body of the uterus was not clearly defined, but seemed to be movable.

November 26, 1870, the tumour was removed in the presence of Drs. Burpee, Drysdale, Mears of Philadelphia, and others.

An incision six inches in length was made, opening the abdominal cavity. The extensive parietal, omental, and vesical adhesions were separated by the fingers, the tumour lifted out of the cavity, and the clamp applied to the pedicle, which was thin, about five inches long, and twisted.

The tumour was multilocular, and consisted of the right ovary. The weight, including contents, between 60 and 70 pounds. The left ovary and uterus were healthy.

Patient recovered well, and is now in the enjoyment of perfect health.

CASE 233. *Multilocular Ovarian Tumour; no Adhesions; Incision four inches in length; a pervious Urachus existing in the Abdominal Wall; Recovery.*—June 8, 1870, Miss A. H., of Altoona, Pa., consulted Dr. Atlee. She was 17 years old, first menstruated at the age of 12, and continued to be regular until March, 1869, when she took cold, and became irregular afterward. The enlargement of the abdomen commenced with the irregularity of the menses, but was accompanied by pain, and developed itself in a general swelling of the abdomen. Latterly she suffered some uneasiness in the right inguinal region. She was considerably emaciated. The abdomen was as large as a woman's at full period of pregnancy, and symmetrical in shape. There was dulness on percussion everywhere, except in the left lumbar region; pelvis free; uterus central; the sound entered 2½

inches. All the characteristic signs of a multilocular ovarian tumour were present. The patient's general health much the same, but the emaciation had progressed, and the abdomen had increased in size.

Diagnosis.—Multilocular tumour of the right ovary.

March 8, 1871, Dr. Atlee removed the tumour. Drs. Fay, Gemmill, senior and junior, of Altoona, King and Benham, of Pittsburg, and others, were present. An incision, three inches long, was made in the linea alba midway between the umbilicus and pubes, and the tumour, which was free from adhesions, was readily removed.

It was multilocular, and consisted of the right ovary, had a very thick cyst-wall, and weighed 25 pounds. The patient recovered.

Remarks by Dr. Atlee.—In this case I found *before* penetrating the peritoneum, and immediately on dividing the muscular wall, a small cyst which contained about an ounce of yellowish fluid. Six days after the operation, when the use of the catheter was omitted, the dressings became saturated with a thin fluid, which exuded from the wound between the sutures. This was found to be urine. On employing the catheter again this discharge ceased, and the wound finally healed. Now, as the bladder occupied its normal position in the pelvis, whence came this urine? The only answer that I can give is that, considering the extra-peritoneal and elevated locality of this urinary cyst, it was a purse in a dilated urachus, which, although closed at the umbilicus, had from within maintained a communication with the bladder.

CASE 234. *Multilocular Ovarian Tumor; no Adhesions; Incision three inches long; Recovery.*—March 22, 1871, Mrs. A. H. G., of Winchester, Va., consulted Dr. Atlee. She was sixty-one years old; first menstruated at the age of fifteen, always regular, and married in her twentieth year. She had ten children and several "false conceptions." The youngest child was born in the forty-fifth year of her age. Her parturitions were easy and recoveries good; lactation free; nursed her children eighteen months, and did not menstruate while nursing. At the age of forty-eight the menses ceased. She always enjoyed good health.

About three years ago she was seized with severe pain in the right inguinal region and right hip, and one year afterwards her friends first noticed an enlargement over the whole lower portion of the abdomen. She had a similar attack of pain, lasting two hours, in February, 1871.

Abdomen as large as that of a woman's at the full period of pregnancy; pretty uniform in shape; relaxed, and had not the usual tension of ovarian dropsy. It was elastic, free from nodules and ridges, and had the most resistance in the left side. The percussion sound was resonant in the epigastric, right hypochondriac, and right lumbar regions. General fluctuation obscure; local fluctuation distinct; indicating two large cysts. On contracting the abdominal muscles, an oval protrusion in the umbilical region was noticed. The pelvis was free, the uterus was central and movable, and the sound entered two and a half inches

Diagnosis.—The physical signs indicate a tumour of the *left* ovary, but the history of the case points to the *right* ovary.

March 27, 1871, Dr. Atlee operated, assisted by Drs. Burpee, Drysdale, Mears, and others.

The tumour consisted of the *right* ovary, was formed of two large cysts, with a small multilocular mass attached to the septum. It weighed twenty pounds. Patient made a rapid recovery.

CASE 235. *Multilocular Ovarian Tumour; Extensive Adhesions; Incision eight inches in length; Weight of Tumour one hundred and twenty-eight pounds; Death on the fourth day.*—May 27, 1871, while at Waterloo, Iowa, Dr. Atlee's attention was called by Dr. A. Middleditch to Mrs. J. S., thirty-nine years old. Menstruation commenced at an early age; married at nineteen, had nine children, the youngest then being four years old. She suffered a good deal in her last gestation and parturition. She first noticed a tumour in the left side. It was quite movable, rolling from side to side. Its growth has been mainly during the last two years. Dr. M. has had her under his professional care, and has attended her through several attacks of peritonitis.

The abdomen was enormously enlarged, measuring round the waist 36 inches, round the umbilicus 62 inches, from sternum to umbilicus 18½ inches, from sternum to pubes 40 inches, and between the superior spinous processes of the ilia 42 inches. When supported in a chair, the abdomen projected to her knees, fell down between the thighs, and rested on the chair below. The lower portion very œdematous. The only resonant points on percussion were in the left border of the chest. Fluctuation distinct in the upper part of the abdomen; lower extremities very œdematous. Emaciation extreme. Pulse 108–12, very feeble.

Uterus elevated; os behind the symphysis pubis, and the sound entered three and a half inches. The pelvis filled by an elastic mass; vagina inverted and somewhat protruding.

Diagnosis.—Multilocular ovarian tumour.

Prognosis.—Unfavourable.

May 28. Ovariotomy was performed, the following gentlemen being present and assisting: Drs. Middleditch, Barber, Lichty, Knox, Eddy, D. F. Crouse, and D. W. Crouse.

After removal of the tumour the abdominal wall was in such great excess that it hung down to the middle of the thighs, and had to be supported on a small pillow. The whole of the wound was below the pubes, and the clamp lay in its upper part, the sutures being below instead of above the clamp.

The tumour was the right ovary, and consisted of one large cyst and a large multilocular mass. Its weight, as reported by the physicians present, who weighed it, with the contents, was 128 pounds. The patient died of exhaustion on the fourth day.

Remarks by Dr. Atlee.—The foregoing case belongs to the second class, when death is impending, and in which an operation offers the only chance of life, while at the same time it lessens suffering, scarcely abbreviating the duration of existence, and may prolong it, even though it cannot save it.

CASE 236. *Multilocular Ovarian Tumour; Extensive Adhesions; Tapped fourteen times; Incision six inches in length; Weight of Tumour ninety pounds; Death on the fourth day.*—June 1, 1871, Dr. Atlee visited Mrs. M. W. at Oakland, near Pittsburg, a widow, forty-seven years old; married before the commencement of menstruation, which occurred at the age of seventeen, and was always regular. She had four children, the youngest being twenty years old. Parturitions were not accompanied by unusual difficulties; lactation free; nursed her children two years, and the menses recurred one year after child-birth. After her first parturition she had an attack of phlegmasia dolens in the left leg, which continued to be affected, more or less, ever after. Menses regular

until the summer of 1869, and continued to return every two or three weeks, lasting eight or ten days, until June, 1870, after which they entirely ceased.

Tumour first noticed July, 1868, and, as she thinks, in the right side. It had a flat or compressed form, and was quite movable, falling from side to side. It has gradually increased ever since. She was first tapped by Dr. Gilmore, November 7, 1869, removing two bucketfuls of fluid resembling soft soap. It was stringy, and by heat coagulated like the white of egg. Afterwards she was tapped thirteen times, the last time on the 20th of May last. There is a sense of obstruction in urination and defecation. Emaciation is very great.

Abdomen of the patient very large, although tapped so recently; tolerably tense and elastic, particularly in the superior portions. Resonant on percussion only in the left hypochondrium. Fluctuation is distinct throughout the upper part of the abdomen, and indistinct below. Here and there nodules can be detected, particularly in the left side. The lower part of the abdominal wall, and the lower extremities are very œdematous. The uterus is elevated and immovable. The sound enters 2½ inches, with difficulty.

Diagnosis.—Multilocular ovarian tumour.

Prognosis.—Unfavourable.

June 2, Dr. Atlee operated, assisted by Drs. Gilmore, Pollock, King, and others.

The tumour was multilocular, consisting of one large cyst, and a large multilocular mass. It was the right ovary, and weighed ninety pounds.

The patient died on the morning of June 5th. Twelve hours after, a post-mortem examination was made, of which Dr. W. L. Foster writes: "Abdomen distended with gas; wound in good condition, externally healed down to the pedicle; peritoneal surface firmly agglutinated; no hemorrhage except a little oozing from posterior surface of womb; ligatures all intact; deposits of lymph over peritoneal surface wherever injured. Left ovary in place, modified, having a pancreatic appearance. Right kidney enlarged and softened; left in healthy condition; liver small."

Remarks by Dr. Atlee.—The above case also properly belongs to the 2d class. The immense size of the tumour, the exhausting tappings, the numerous vascular adhesions, and the emaciation, properly place it in this position.

ART. XI.—*Case of Congenital Retinitis Pigmentosa.* By GEO. C. HARLAN, M.D., Surgeon to Wills Ophthalmic Hospital.

A. C., an intelligent girl, 18 years of age, had very imperfect vision. She stated that she had never seen better, indeed that her vision had improved since childhood. There had never been any pain or other sign of inflammation in either eye. She was sent for examination because her friends noticed that she often retired to some distance to look at an object, and were in hopes that glasses might be found to improve her

near vision. She herself insisted, however, that she saw better near at hand when she had once found out just where to look for an object, but that it was often necessary to step back for some feet before she could fix it. A single flower on a bush, for instance, was seen much more distinctly when quite close to the eyes, but she was sometimes unable to see it at all until she had first taken a more distant view of it, and then walked towards it.

She could distinguish No. cc of Snellen's type at twenty feet, though not very readily, and could read No. 7, with difficulty, at one foot. Convex glasses made no improvement. Vision was better in a very bright light, as sunlight falling directly on a page of large print; and she experienced difficulty in going about after sundown. In other words, she had decided hemeralopia. There was quite a high degree of nystagmus.

On testing the field of vision, it was found very much contracted, which at once explained the apparent anomaly in her symptoms. A small object completely occupied the sensitive part of the retina, and could be seen only when its image fell exactly on the macula lutea. At a distance, the apparent size of objects being less, a greater number of them were included in the field.

Ophthalmoscopic examination showed the characteristic appearances of "retinitis pigmentosa," or, perhaps, more correctly, "pigmentary degeneration of the retina," very well marked.

There was no consanguinity between her parents, no history of hereditary blindness, and no indication of hereditary syphilis.

As neither the refraction nor accommodation was at fault, of course no form of spectacles could be of use, but as the retina, even in its contracted field, was incompletely sensitive, the patient was directed to procure a large convex hand-glass to magnify the letters of ordinary print to a size that would enable the eye to distinguish them.

An interesting point in the case is that, contrary to the usual course of the disease, which is a gradual but constant contraction of the field of vision, tending ultimately to complete blindness, the patient positively asserted that there had been a decided improvement in her vision.

ART. XII.—*Anatomical Anomalies.* By T. HALE STREETS, M.D., Assistant Surgeon U. S. N.

MOST of the following anomalies were observed in the dissecting-room of the Philadelphia School of Anatomy, during the session of 1871–72. They were collected at the instance of Dr. W. W. Keen, and are published with his consent.

Osseous.—A female subject which presented two supernumerary ribs in the cervical region. They articulated with the seventh vertebra, one on each side. It is to be regretted that the parts in this region were destroyed before their relations could be noted by a competent observer. Dr. Keen has the specimen and will describe it more fully.

Muscular.—A subject in which a muscular slip arose from the fascia

which binds down the flexor sublimis digitorum, about two inches above the pisiform bone. Its origin was about half an inch in breadth. The fibres converged as they passed downward, and terminated in a long, narrow tendon, which joined the abductor minimi digiti, at its origin, at a very obtuse angle. The tendon then ran down along the outer border of the abductor and gave attachment to some of its fibres. It was inserted into the same point on the base of the first phalanx of the little finger as the abductor minimi digiti, and in action antagonized, in a measure, this muscle. It also assisted in flexing the little finger.

A case in which the tendon of the flexor profundus digitorum did not pierce the tendon of the flexor sublimis digitorum on the little finger, but passed to the radial side of it. This was observed on the little finger of both hands.

Arterial.—A subject in which the ascending pharyngeal was given off from the inner side of the occipital.

In the same subject which was mentioned above as presenting two supernumerary cervical ribs, the vertebral artery of the right side was small and entered the vertebral foramen of the fifth cervical vertebra; while that of the left side entered the foramen of the fourth cervical vertebra.

A subject in which an axis existed on the brachial at the lower part of the upper third of the arm, which gave off the superior profunda, the nutrient, and the inferior profunda arteries. In another case the brachial gave off the inferior profunda somewhat lower down than usual. It was as large as the brachial below the point of its origin. It ran down in the triangular space formed by the pronator radii teres and supinator longus, and then passed beneath the flexors in company with the median nerve, and gave off the interosseous. The brachial, much reduced in size, bifurcated at the bend of the elbow into the ulnar and radial, which were quite small. The former passed over and lay superficially to the flexor muscles. It crossed the pronator radii teres at its upper third, the flexor carpi radialis at its middle, and then ran parallel with the tendon of the flexor sublimis digitorum to the palm of the hand.

A subject in which the brachial of the right side gave off, from its anterior surface, about one inch above the point of its usual bifurcation into the ulnar and radial, a small branch which passed down the middle of the forearm on the outer side of the flexor carpi radialis. It crossed the tendon of this muscle and terminated on the tendon of the flexor sublimis digitorum just before it passed under the annular ligament. In another subject the same anomaly was observed. In this case the median branch ran down in company with the median nerve, and was distributed to the palm of the hand. In two cases the brachial artery was observed to bifurcate along the humerus; once at its upper third, and once at its middle.

A case in which the radial gave off the superficiales volæ, on the right side, two inches above the lower end of the radius, and, on the left side, three inches from the same point. In both instances it was as large as the continuation of the radial.

A subject in which the hepatic artery gave off a branch to the œsophagus. The branches from the gastric were very small. In the same subject the gastro-epiploica dextra came off as a separate branch from the superior mesenteric, and the colica media and colica sinistra were given off by a common trunk from the inferior mesenteric.

A subject in which an accessory pudic artery was given off from the obturator of the right side. After passing forward for about one inch, it

divided into two branches, one of which formed the artery of the corpus cavernosum, and the other the dorsal artery of the penis. No middle hemorrhoidal was given off from the anterior trunk of the internal iliac. The sciatic was large, and divided into two branches; the anterior of which supplied the place of the middle hemorrhoidal. On the left side the obturator was given off from the deep epigastric.

A subject in which an abnormal artery was given off from the femoral. On the left side it arose about midway between the superficial circumflex ilii and profunda femoris. On the right side an artery analogous in function arose from the profunda femoris just below the origin of the external circumflex. Both of these arteries came off from the external surface of their parent trunks, and afterwards divided into three branches, which supplied the vastus externus, the rectus femoris, and cruræus. The same anomalous artery was observed in another subject on both sides. In this instance they both arose from the profunda about half an inch below its origin.

A case in which the anterior tibial was wanting.

Nervous.—Four cases were noted in which the musculo-cutaneous nerve gave off a communicating branch to the median in some part of its course. In one instance it gave a large communicating branch just before it pierced the coraco-brachialis muscle. In two instances it gave off a smaller branch at the lower third of the arm. Here the communicating branch crossed the brachial artery. In the fourth case the communication took place at the upper third of the arm.

A subject in which the nerve of Wrisberg was given off as a branch of the internal cutaneous.

ART. XIII.—*A Peritoneal Inflammatory Cyst resembling an Ovarian Tumour; Exploratory Operation; Incision Five or Six Inches long; Death on the Third Day.* By WASHINGTON L. ATLEE, M.D., of Philadelphia.

APRIL 25, 1871, I visited at Platte City, Missouri, Mrs. G. W. B., at the request of Dr. J. T. Wilson, of Weston, Mo.

The physician of the patient, Dr. Johnson, informed me that at the time of parturition, five months before, he felt a tumour on the right side of the uterus, and perfectly movable and elastic as if it contained fluid. After the birth of the child—even through the fat walls of the abdomen—he could press his hand edgewise between the tumour and the uterus, and move them readily on each other. Some days after childbirth she was seized with rigor, followed by fever and supposed peritonitis, and a rapid development of the tumour, with great constitutional disturbance and emaciation.

At the time of my visit the patient was larger than a woman at full period. The shape was pretty uniform, the abdomen elastic and readily fluctuating in all directions. The percussion sound was resonant over the whole left side, and dull elsewhere. The pelvis was filled with an elastic tumour, and the uterus was elevated above the brim of the pelvis entirely beyond reach. Externally its body could be traced near the umbilicus to the left of the median line, and about one and a half inch from it.

Notwithstanding the case was very unfavourable for operation, in consequence of the acuteness of the original attack, the rapidity of development, the pelvic complication, the displacement of the uterus, and the general condition of the patient, I agreed to make an operation of exploration, believing at the time that the tumour was ovarian.

The following gentlemen were present, and assisted in the operation: Drs. Johnson and Beaumont, of Platte City; Bonifant, Wilson, and Shortbridge, of Weston; Robinson, of Ridgely, and Coffey, of Camden Point. I made an incision in the linea alba and carefully opened the peritoneal cavity, so that I could examine the interior. The uterus was found to occupy the position above stated, its fundus being above the umbilicus. The broad ligament of the right side was stretched obliquely upward across the whole body of the tumour, in front of it and grasping it like the open hand. The left broad ligament was stretched in like manner obliquely downward, but to a smaller extent, across the left side of the tumour. The fundus of the uterus, and the upper edges of the two broad ligaments, formed a distinct ridge, extending across the tumour obliquely from the left inguinal region to the right hypochondrium. In the upper border of the right broad ligament was a healthy elongated ovary, with a cyst as large as a hazel-nut in its distal extremity, and in the corresponding part of the left broad ligament was the elongated healthy left ovary. The pelvic peritoneum was raised out of the pelvis by the tumour so as to form a cul-de-sac of reflected membrane about midway between the umbilicus and pubes, extending across the whole front of the abdominal cavity. I now extended the incision to the distance of five or six inches, and passed my hand into the cavity of the abdomen, in order to ascertain more clearly the relations of the tumour. Satisfied that the tumour was not ovarian, and that it involved in its walls all the viscera of the lower part of the abdomen and of the pelvis, I abandoned the idea of its extirpation. It being cystic, however, I passed a large trocar into it above the left broad ligament, near the uterus, and drew off fifteen pints of greenish pus and numerous large, stringy clots of fibrinous matter. The small cyst in the right ovary was also punctured, and a thin, transparent fluid escaped. A tent was now placed into the cyst and carried out of the lower edge of the wound, and the latter closed with five or six sutures. The patient died on the third day.

Remarks.—The foregoing case was one of very peculiar interest. Before the operation I diagnosticated the tumour to be ovarian. Afterward I supposed that the original tumour was a pedunculated uterine fibroid tumour, that inflammation had supervened, and that an abscess had formed within it, which had caused it to partake of the characteristic signs of a unilocular ovarian cyst. Subsequent experience, however, satisfied me that this last opinion was erroneous, and that the tumour originated in the pelvis through inflammation localized in the peritoneum, agglutinating the serous surfaces and forming pockets therein to receive the resulting fluids of this inflammation, forming what I denominate, in my book on the "Diagnosis of Ovarian Tumours," a *peritoneal inflammatory cyst.*

REVIEWS.

ART. XIV.—*Electro-therapeutics.*

1. *A Treatise on Localized Electrization and its Applications to Pathology and Therapeutics.* By Dr. G. B. DUCHENNE. Translated from the third edition of the original by HERBERT TIBBITS, M.D., Licentiate of the Royal College of Physicians of London; Medical Superintendent of the National Hospital for the Paralyzed and Epileptic. With numerous illustrations and notes and additions by the translator. Philadelphia: Lindsay & Blakiston, 1871.
2. *A Practical Treatise on the Medical and Surgical Uses of Electricity, including Localized and General Electrization.* By GEORGE M. BEARD, A.M., M.D., Fellow of the New York Academy of Medicine, Member of the New York County Medical Society; and A. D. ROCKWELL, A.M., M.D., Fellow of the New York Academy of Medicine, Member of the New York County Medical Society. With one hundred and two illustrations. New York: William Wood & Co., 1871.
3. *Lectures on the Clinical Uses of Electricity, delivered in University College Hospital.* By J. RUSSELL REYNOLDS, M.D., F.R.S., Professor of the Principles and Practice of Medicine in University College, Examiner in Medicine to the University of London, Physician to University College Hospital. London: J. & A. Churchill, 1871.
4. *Galvano-therapeutics. The Physiological and Therapeutical Action of the Galvanic Current upon the Acoustic, Optic, Sympathetic, and Pneumogastric Nerves.* By WILLIAM B. NEFTEL, M.D., New York: D. Appleton & Co., 1871.
5. *Electricity in its Relations to Practical Medicine.* By Dr. MORITZ MEYER, Royal Counsellor of Health, etc. Second revised and corrected American edition. Translated from the third German edition, with notes and additions, by WILLIAM A. HAMMOND, M.D., Prof. of Diseases of the Mind and Nervous System, etc., in Bellevue Hospital Medical College, etc. New York: D. Appleton & Co., 1872.

THE list of works on electro-therapeutics is steadily lengthening. This fact indicates the increasing attention which is being devoted to the subject, and gives hope for further solution of the mysteries which are still bound up in the subtle science of electricity. It cannot but be expected that there should be considerable repetition in medical books which appear almost at the same time upon the same subject; but light is, nevertheless, thrown upon it, for it is regarded by different writers from different points of view. One looks mainly at the scientific side, the other at the practical, and thus the science and art are simultaneously developed and new thoughts suggested. As physicians, also, we cordially welcome all efforts

which result in wresting healing agents from the hands of ignorant or unprincipled men. No doubt there is still something of empiricism in the use of electricity by even those who have laboured earnestly and honestly to find out the indications for its employment. There is much about this agent which is still unknown, and consequently it is not always an easy matter to decide as to its application. Yet this is true of nearly all, if not all, of our remedies, but it does not deter us from using them with a good degree of confidence. Besides, a physician, who in a spirit of humanity and scientific zeal, is working for the relief of suffering and advancement of knowledge, is almost certain to advance the science of medicine, while the charlatan labours for gain, and science is held of little account. This question as to the uncertainty of electro-therapeutics is thus referred to in one of the works now under consideration, in a chapter from which is taken the following extract:—

"The truth is, that among the advanced minds of our day the conviction is ever deepening and strengthening that therapeutics is not yet, and may never be, an exact science; that the time may indeed never come when it shall be possible to treat any disease by the rule of three. So profound is our ignorace of the chemical constitution of the body; of the molecular and other changes that incessantly take place in health and disease; of the modifications which these changes undergo by the infinite and varying influences of climate, temperament, diet, and mode of life; and so limited and uncertain is our knowledge of the nature and action of medicine, that we may well resign the hope of reducing therapeutics to an exact science, to distant generations. The number of those in the profession who will subscribe to the emphatic words of Niemeyer, 'It is idle to hope for a time when a medical prescription should be a simple resultant of a computation of known quantities,' is rather increasing than diminishing with the advance of science, and the belief is extending that therapeutics is mainly a matter of empiricism; that while we are waiting and toiling to perfect our knowledge of pathology and determine its relation to therapeutics, we must not allow our patients to suffer, but should faithfully use those remedies and systems of treatment that experience has recommended.

"Such has been the practice of many of our most successful experimenters with electrization."—*Beard and Rockwell*, p. 115.

Our own conviction is, that, while there is much empiricism in the application of remedies, the case is not so bad as would be inferred from the foregoing quotation. Given a fair knowledge of remedies, the results will be almost directly in proportion to correctness of diagnosis. It is, however, at present pretty generally conceded by physicians that electricity is a valuable remedy. This concession is comparatively recent, and many would still dwell quite as readily upon its uncertain action as upon its brilliant cures. But admitting it to be a remedy of acknowledged value, it is then a question as to the kind which shall be used and the method of application. Duchenne regards the faradic current as *par excellence* the one adapted for medical purposes; others prefer galvanism, but the best opinions regard both as useful, each to be employed with a fair degree of precision for the fulfilment of certain indications. There is a difference of opinion as to the utility of franklinic electricity. Duchenne, who may be called a *partisan* of faradism, says:—

"It appears then, on the whole, that the excitation of muscle by static electricity ought generally to be excluded from practice, and the more so since it may be replaced by another electric agent, which will be described in the third section (induced electricity); and which excites muscular contractility and sensibility in a more energetic and effectual manner than the former, without

any of the inconveniences that I have described. I purpose to point out in the sequel the cases in which its employment will be useful or necessary."

"It is, however, incontestable that static electricity, which for many years was used exclusively in medical practice, has wrought some cures seemingly marvellous. These results show only that certain paralyses are always cured by electricity, in whatever form and manner it may be employed. Unfortunately, these cases have been so rare that this mode of using electricity has not borne the test of time, and has been many times laid aside by physicians after having been taken up with some degree of enthusiasm." Further on, however, at page 44, he says: "In my hands static electricity has been of great utility in certain cases in which the other varieties have been insufficient." (pp. 8–9.)

Dr. Tibbits, who has translated Duchenne's works, and who is medical superintendent of the National Hospital for the Paralyzed and Epileptic, states that franklinic electricity has been found of considerable value in that institution, as well as in private cases occurring under his own observation. Facial neuralgia, facial spasm, hysterical aphonia, hysterical hyperæsthesia, and general or local tremor are the diseases especially mentioned as having been benefited by insulation, or drawing sparks from the affected regions.

Dr. Russell Reynolds also speaks unreservedly in favour of its use in certain cases, but Drs. Beard and Rockwell are faint in their praises, while admitting that Professor Schwanda, of Vienna, has reported favourable results obtained by means of Holtz's electrophorous machine. It seems to be admitted then that franklinic electricity is valuable as a therapeutic agent; it is not yet decided whether it is superseded in all cases by faradism or galvanism.

Passing to the consideration of faradism we are in the presence of an agent which has been in use for many years, has achieved unexpected success, has often been the cause of disappointment, but has slowly gained ground, and with improved methods for appliance will probably take a permanent position in therapeutics. But what do we know of faradism? Those who have used it to any extent would pretty generally agree that its effects are mainly, what for want of a knowledge of essential properties, we term stimulant and alterative. By its aid nervous and muscular vitality are increased, and pain is sometimes relieved. Duchenne also considers that the current which proceeds from the first coil of wire, or that which immediately surrounds the soft iron needles, excites more acutely the nerves, muscles (proportionately increasing their contractility), rectum, bladder, testes, epididymis, and spermatic cord, and that the current of the second coil acts more powerfully upon the cutaneous sensibility, the sensibility of the retina, and penetrates most deeply into the tissues. He also believes that the therapeutic action of localized faradization, to which method of application he was such a devoted adherent, depended upon the promotion of general circulation which resulted from its use, and probably also from the excitation of the *trophic* nerves which govern nutrition. He seemed to be of opinion that faradism could produce all of which galvanism was capable with the exception of catalysis, but this is not the general opinion. As has been previously observed, the faradic current is more generally regarded as a nervous and muscular stimulant when mildly administered, but capable of producing directly opposite effects when very strong or protracted. It differs from galvanism, as a rule, in the absence of the production of those central and chemical phenomena which are characteristic of the battery current.

Making this distinction, we must now consider the galvanic and faradic

current together, since it is a question whether the difference is not, to use a medical term, functional, rather than essential or organic. Duchenne thought that localized muscular galvanization by interrupted currents, applied with a battery of sufficient electro-motor power, produced a therapeutic action almost identical with localized muscular faradization. He also explained as follows those cases in which muscles will not respond to the faradic current, but are affected by galvanism:—

"The muscles which in certain forms of paralysis no longer contract under the influence of localized faradization, may sometimes still be thrown into contraction by the intermissions of a voltaic current, when these muscles or their nerves have been traversed, for one or two minutes, by a continuous current.

"This phenomenon, which for the first time was described by Remak, in pathological conditions, had been previously experimentally discovered in animals, by R. Heidenhain, who observed that on submitting a muscle to the shocks of induction, or on tearing it by stretching, or on plunging it into hot water (28° to 30° Cent.), it lost its excitability; and that this property (the faculty of contracting under the influence of electric excitation) was restored to it by the continuous current of a Daniell's battery of 25 or 30 elements, in a manner more evident in employing the ascending than the descending current.

"I have tried a very large number of times to produce this electro-muscular phenomenon, in traumatic paralysis of nerves, in saturnine paralysis, and in the spinal paralysis of infancy, but I have only seldom succeeded.

"I should here point out the following source of error. When the contractility is considerably weakened, a time comes when the muscle no longer contracts under the influence of the rapid intermission of induction currents, while it still responds, in a very appreciable manner, to the more distant intermissions (from one to two seconds) of the same induction current. But, a great number of induction instruments giving none but rapid intermissions, it has happened that on applying them to the investigation of electro-muscular contractility, this has been supposed to be abolished, while it would still have been called forth by the slow intermittences of an induction current and even of a galvanic current. It has been by proceeding in this way, there can be no doubt, that experimentalists have been deceived by appearances, and have been brought to believe in the frequency of the electro-pathological phenomenon now under consideration.

"However this may be, I repeat that I have sometimes found cases of paralysis in which the muscles no longer responded to the distant intermissions of an induced current, but in which they contracted more or less under the influence of the distant intermissions of a strong voltaic current, after having been traversed during from sixty to eighty seconds by a constant continuous current." (pp. 195, 196.)

We may also infer, in the absence of actual proof, that the different varieties of electricity are radically the same, from the fact that the diseases in which they have proved curative are all eminently of a similar character.

With regard to the action produced at the different poles, and the direction of the currents, it seems to be pretty well agreed upon, that although experiments on animals and chemical investigations show that lowered irritability, the dilatation of vessels and the presence of acids are associated with the descending current and the positive pole, and the reverse phenomena with the ascending current and negative pole, yet these distinctions are not to be closely regarded in practical therapeutics. Says Dr. Reynolds:—

"Supposing I have the positive end of the battery connected with a person's left hand, and the negative end connected with his right, the current passing from

the positive to the negative pole, goes up the person's left arm to the trunk of the body, and down the right arm to the machine again. The current passing up the arm has been called the 'inverse,' and that coming down the arm has been called the 'direct.' In the arm in which the current is passing upwards the 'irritability' of the muscle and nerve is gradually increased; in the other arm, in which the current is passing downwards, the irritability is gradually diminished. You may test this fact by now and then breaking and remaking the continuous current, and you will find that in the two arms two different degrees of irritability exist, according to the direction in which the current has been passing, whether up or down. One arm will act more, the other less readily than in health. The difference thus produced between them is sometimes highly marked.

"It may occur to you to ask, 'If that be the case, which current should I use to relieve pain and spasm, the direct or the inverse?" All I have to say is, that, so far as I have seen, it does not make the smallest difference. Theoretically it should make a very great difference, but practically, so far as my observations extend, it makes none. I have seen pain or spasm relieved as well by the current in one direction as in the other, and this whether the spasm has been clonic or tonic, or whether there has been merely tremor." (pp. 9, 10.)

Beard and Rockwell write in a similar manner:—

"The rule is that the descending current calms, while the ascending irritates; and yet there is no question that both currents may have a calming or an irritating effect, according to the strength of the current used, the length of the application, and the nature and locality of the disease.

"Some operators apply the positive pole over the seat of the pain or inflammation, and others the negative; and both obtain good results. Some use the ascending current, others the descending; and both produce satisfactory muscular contractions. In localized galvanization of the brain, cervical sympathetic, and spinal cord, both directions of the current produce therapeutic results." * * * * "Whether the effect of an application of electricity is of calming or irritating character, depends on the temperament of the patient, the nature and locality of the disease, and the length and strength of the application, as much as, oftentimes even more than, on the position of the poles or the direction of the current." (p. 165.)

Duchenne objects with regard to some of the effects claimed for electricity that they are deduced from experiments on animals, and that the reaction in man is different: perhaps improved and more extensive experience will reconcile some of the apparent discrepancies between theoretical and practical electricity. As scientific men such a reconciliation would certainly be more satisfactory to us. Probably Dr. Neftel's statement that the "galvanic current *alone* exercises a direct action upon the organs of special sense, as well as upon the nervous centres," is rather broader than a physician of the author's attainments would be willing to endorse, since there is evidence that the faradic current will, although in rare instances and less decidedly, affect nervous centres. He also believes, with Remak, that "the general effect of a prolonged application of the galvanic current is soothing, with an increase of temperature and subsequent perspiration, and is almost always followed by a prolonged, refreshing sleep." This sedative effect of galvanism is, we think, pretty generally admitted.

In the preceding paragraphs an effort has been made to present the general views of electricity, medical electricity, on which there is least disagreement among those who have studied the subject carefully, as well as to hint at the indications for the use of the different varieties. Some points will be subsequently referred to in a rapid review of the different works singly, on which there is a decided difference of opinion. Special directions as to the employment of electricity in a given list of diseases

have not been offered here, for they would not fall within the province of this article.

In reading Duchenne's work, which Dr. Tibbits has made accessible to all readers of the English language by a smooth and finished translation, we are struck with its scientific character. First comes the statement, and then the proofs by which the claim is believed to be established. Where, as in the inquiry, as to what class of nerves promote nutrition and capillary circulation, he depends more upon hypotheses, it is in the tone of one who apologizes for such support. Thus he says :—

"It is with fear that I approach this question. In all my earlier researches into the physiology of movement the problems to be solved have always been in harmony with anatomy, with electro-muscular experiments, and with clinical observation. On the contrary, in the new path on which I am now to enter, I cannot make a step without proceeding from hypothesis to hypothesis, relying only upon the great discoveries of experiment."

Such sentiments inspire confidence, yet in those parts of the work which refer to Remak and the continuous current, we observe an adverse prejudice which is not well concealed. In fact, the work is defective in reference to galvanism, and to supply this want Dr. Tibbits has appended a condensed translation of Ziemssen's views upon the subject.

Dr. Duchenne commences his work by a description of the different varieties of electricity, touches upon static or franklinic electricity, gives a short space to galvanism, and passes on to faradism. In the portion on galvanism, he asks :—

"Is the continuous current really an hyposthenisant of nervous force? If we may apply to mankind the results obtained by vivisection, the reply should be in the affirmative. We know in fact, that a continuous current, passed for a certain time through the nerves or the limbs of an animal, not only diminishes the motor power, but even produces paralysis. I shall show in the sequel by the results of experiments that this kind of hyposthenisant action is not appreciable in man. With regard to the action of the continuous current upon sensation, although after a certain time it produces numbness of the limb through which it passes, I do not believe that it can be considered as an hyposthenisant of the nervous system, or, in other words, as an anæsthetic." (p. 13.)

This is one of those points on which there is a difference of opinion, and we think that a majority of those who have used the continuous current would be disposed to admit its anæsthetic or soothing influence. Our own experience, added to that of others, leads us to this admission, and we believe we have observed a diminution in the pain of malignant disease as a consequence of the application of galvanism. Others have noticed this effect, and in the April number of this Journal for the present year, on page 465, is a report of an interesting case of encephaloid disease for which galvanism was used. The report was communicated by Dr. Althaus.

"This case is one of very great interest. The effect of the electrolytic treatment, which was not employed until late in the history of the case, was, except upon the first occasion, to afford complete relief from pain, for four or five days after each application, and to slightly diminish the size of the tumour. The intensity of the pain, before the application of electricity, may be inferred from the fact that it had been found necessary to administer hypodermically from twenty to twenty-four grains of morphia in each twenty-four hours. We quite agree with Mr. Tait that electrolysis is worthy of a fair trial in any case of cancer in which operative interference is not deemed advisable."

Duchenne's experiment consisted in passing a current of from thirty to fifty elements through the body of a healthy man who had previously been

subjected to the passage of a current from sixty pairs. He thus describes it:—

"It has been shown by numerous experiments upon animals, that the continuous current manifests its special influence upon contractility, or upon sensibility, according to the direction in which it passes longitudinally within the nerve, when the excitability has been previously diminished by the more or less prolonged action of a continuous current.

"To discover the influence of a continuous current upon a healthy man, it was therefore necessary that the excitability of his nerves should be first modified by the continuous current. For this purpose, I employed in the first instance, a trough battery (of Cruikshank and Wollaston) composed of sixty couples, and I caused a continuous current to pass through the nerves for from twenty to thirty minutes, by placing them in the circuit of this battery, and by using the method of procedure indicated above. This attempt having produced no result, I thought that a more powerful and more constant current might diminish the excitability of the human nerves, and I then had recourse to a battery composed of thirty of Bunsen's pairs, a little weakened by previous use. Afterwards I employed a Daniell's battery of from thirty to fifty elements. In all these experiments, I took care to avoid the occurrence of contraction in completion of the circuit, by proceeding in the way formerly described.[1] After twenty or thirty minutes of action of the continuous current from these batteries, in whatever direction, I was unable to discover that the sensibility of the nerves had diminished. The contractions of the muscles supplied by the nerves were quite as energetic, under the influence of an intermittent current, after as before the experiment; the sensibility was not modified, and the power of voluntary movement was not diminished.

"Although I was unable to diminish artificially the excitability of human nerve, I had at least occasion to observe certain instances of paralysis, in which the nerves and even the muscles had in part lost their excitability. In these cases again, the inverse continuous current acted no more upon the contractility than did the direct continuous current upon the sensibility. Neither of these currents abolished the excitability of nerves already affected, and through which they were made to pass for a considerable time.

"To sum up, in a man in normal condition: 1. A continuous current passed through a nerve, during from twenty to thirty minutes, did not appear to me to diminish its excitability; 2. When I directed a continous current, centrifugal or centripetal, upon a nerve of normal excitability, or upon a nerve the excitability of which was diminished by certain forms of paralysis, I observed always the customary phenomena, namely, contractions and sensations; neither the one nor the other of the two (direct and inverse) currents ever acted in an appreciable manner, upon either the sensibility or the contractility separately."[2] (pp. 176, 177.)

What is to be done when in questions of therapeutics the authority of great names is attached to conclusions at variance with our own experience, but to believe that often both the conclusions, although differing, may yet have been true in the cases under consideration. This is frequently observed with reference to the effects of a particular drug in the hands of different physicians.

The illustrations given of the different effects produced by the currents of the first and second coil would seem to confirm his statement that the two currents should not be used indifferently. And it must be noticed by any careful reader of this scientific work that the claims made throughout

[1] Placing a tube, filled with water, called a moderating tube, in the circuit.

[2] Duchenne's note to the foregoing—"I formulated almost the same conclusions in a memoir presented in 1848 to the Académie des Sciénces. But at that time I had not endeavoured, in my experiments, to diminish the irritability of nerves by subjecting them to the influence of a powerful continuous current."

the volume for the varieties of electricity and methods of application, are the result of close observation and experience.

In considering the fundamental principles of his system of localized electrization, he seeks to localize electricity in the skin without influencing deep-seated organs, or, on the other hand, to concentrate its power upon a nerve or muscle and make it penetrate to subjacent tissues. He finds his solution of this problem by the use of dry or moist rheophores, the dry limiting the action to the skin, the moist causing the fluid to penetrate more deeply. This subject is considerably elaborated; then follows a description of different instruments, and it may here be mentioned, that in another part of the work, in speaking of the necessary requirements of an induction apparatus, he considers that it should possess primary and secondary coils, constructed after certain proportions of thickness and length of wire, and adjustments for producing slow or rapid intermissions, as well as for measuring and distributing powerful or feeble currents; these requirements can scarcely be questioned.

Following the description of instruments come directions for the method of practising muscular faradization. When the electric excitation of the muscle is accomplished by passing the current through the nerve distributed to it, the process is termed *indirect muscular faradization;* when an individual muscle, part of a muscle, or fasciculus, is excited by acting directly upon it with the current, it is the method of *direct muscular faradization.* In these operations the rheophores must be brought as near as possible to each other without touching, probably with a view of confining the action to the immediate vicinity of the rheophores, since in another part of the work he says that, if the operator be careful to keep the rheophores together, it is certain that the current is felt but feebly by the nervous centres.

In the midst of the directions now given for the electrization of special organs, Dr. Tibbits has inserted an extensive quotation from Ziemssen on laryngeal electrization, in which that subject is very fully considered, and is worthy of careful reading.

Chapter III. is devoted to historical and critical observations upon the principal methods of electrization, and here are considered: 1. electro-puncture; 2. electrization by reflex action; 3. localized electrization by induction currents and by interrupted galvanic currents; and, 4. continuous constant currents.

After a historical retrospect of the subject of electro-puncture, he expresses the opinion that it is far from the attainment of the end proposed by Sarlandière and Magendie. His objections to it are that it does not exactly localize electricity in those organs intended to be operated upon, that it is insufficient or inapplicable in anæsthesia or lesions of tactile sensibility, and inferior to localized faradization in the treatment of muscular paralysis. He states, however, in a note, that it has been of service in hydrocele; in the resolution of certain ganglionic tumours; in destroying certain hepatic and ovarian cysts, and in forming a coagulum in aneurism.

Premising that, according to the law which governs the conduct of the nervous fluid in the motor nerves, the motor action is in the direction of those nerves which are distributed to muscles, it is stated that

"Electrization of the nerve-terminations of limbs will never call into contraction muscles which receive their nerve filaments from branches given off above the point excited." (p. 221.)

If, for illustration, two rheophores are held in one hand without coming

into contact, the muscles of the forearm do not enter into contraction. On the other hand, if one rheophore is held in the hand and the other placed higher up on the limb, or if one is held in each hand, the contraction of the muscles of the forearm or arm takes place according to the position of the rheophores and intensity of the current. He therefore concludes that

"In order to produce contractions of muscles situated above the nerve-terminations, it is necessary to separate the rheophores in such a manner that the currents may traverse the limbs, or rather their nerves, in a longitudinal direction; this is *electrization by reflex action*." (p. 121.)

The electrization by reflex action operates specially upon the sensibility, ascends to the posterior roots of the nerves, affects the anterior roots, and thus produces, in the muscles dependent upon the nerves the terminations of which have been excited, irregular contractions that are but little in harmony with the intensity of the sensations.

This method the author considers applicable to the treatment of some forms of paralysis in which it is proper to stimulate the nervous centres. It is not, however, very warmly recommended, since while some cases have been benefited, others have suffered after the treatment, from deep-seated pains in the course of the nerves, the extremities of which had been stimulated. Serious consequences have also resulted in certain instances. He recommends localized faradization in preference, but even this is not highly praised. He does, on the other hand, recommend the passage of mild induced currents from the mouth to the anus as an effectual remedy in asphyxia, since thus the general reflex action of the cord is most certainly excited. Cutaneous faradization of the precordial region is also recommended in asphyxia, and in atrophic paralysis localized faradization is considered of great value. In support of his views on atrophic paralysis, reference is made to the experiments of our countrymen, S. Weir Mitchell, G. R. Morehouse, and William W. Keen, on "Gunshot Wounds and other Injuries of Nerves."

It would require too much space to give in full, the observations commencing on page 149 with regard to the comparative effects of induced and continuous currents on the sympathetic nerve. In the first place the author objects to the experiments of Onimus and Legros on rabbits, which consisted in laying bare the nerve in the neck, slightly lifting it by means of a thread placed beneath, and then electrifying it for two or three minutes by powerful induced currents. "The artery of the corresponding ear became contracted until it was no longer perceptible. On ceasing the electrization, the artery regained its size little by little; and finally became larger than it was before the operation. After an instant, the temperature of that side was elevated above that of the other." Duchenne considers that the dilatation of the vessels of the ear was produced by paralysis of the vaso-motors, but that the current used was too powerful, and he asks whether there is not some cause for surprise that the sympathetic, already exposed to the air and perhaps more or less injured in the dissection, was not killed by the strong current, and that the vessels could again regain their normal calibre. He says:—

"To proceed thus with experiments the object of which is to study the action of induced currents upon the nervous system, is but to contribute to toxicology. We know that there are certain poisons which, in small doses, increase the action of the heart, but which would arrest that action if given in large doses. To expect a therapeutic action from such a manner of applying induced cur-

rents is like maintaining that paralysis may be as readily cured by a thunderbolt as by electricity wisely and methodically applied."

"There is certainly no need of experimental medicine to inform us that in its therapeutic aspect, localized faradization is a two-edged weapon. Empirical clinical observations very soon convinced me, at the beginning of my electrotherapeutical researches, that localized faradization, if applied to a muscle or nerve in too full a dose, or for too long a time, may increase or even produce paralysis or atrophy, instead of curing either. It was such clinical observation that led me to lay down the precept that muscular faradization should be moderate in degree, and that neither the whole application, nor the application to any single muscle, should be unduly prolonged."

"With a powerful induced current contracting the vessels during its passage through the sympathetic, and producing, after its cessation, a temporary diminution of their contractibility (and hence their dilatation), the same authors have compared the relatively infinitely feeble action of continuous currents upon the same sympathetic, 'under the influence of which,' they say, 'the activity of the circulation was promoted during their passage, and for a long time after their cessation.'" (p. 151.)

Further on he says—

"Partisan and defender, as has been seen already, of the existence of different physiological properties in the different kinds of electricity, although the reason of such difference has not at present received complete explanation from actual physical data, I was disposed to hail the discovery of a special difference of action between the continuous and the induced currents as a triumph of experimental medicine, applicable to therapeutics. But I must add that there remained in my mind some doubt with regard to the reality of the assigned difference; because in the comparative experiments, the forms of current had not been applied in equivalent force.

"It was, therefore, important to verify the results said to have been attained, and the following is a summary of experiments made upon rabbits by myself, in conjunction with my learned friend M. Liégeois, Professor of Physiology to the Paris Faculty of Medicine. Our results are far from agreeing with those of MM. Onimus and Legros; and show that, in equivalent doses and under similar conditions, the continuous constant and the induced currents, when passed through the great sympathetic, exert an identical action upon the vasomotors.

"*Experiments.*—A continuous constant current (from a pile of from fifteen to thirty sulphate of lead elements) passing through the sympathetic in a portion three or four millimetres below the superior cervical ganglion, produced a slight but manifest contraction of the vessels, and a decoloration of the ear on the same side, during the whole time of the experiment.

"When the sympathetic nerve had previously been strongly excited, or, in other words, when its excitability had already been more or less exhausted, as by the passage of a powerful induced current, or by section, the same continuous current of thirty elements, or even of fifteen, produced, instead of contraction of the vessels, their dilatation during the whole time of the experiment.

"An extremely weak induced current (the minimum of the extra current of my volta electric apparatus passed through a column of distilled water twenty-two centimetres in height), and localized in the sympathetic in the same manner for three minutes, produced a slight contraction of the vessels of the ear during the first half minute, followed by dilatation during the rest of the experiment.

"The same proceeding, repeated upon the sympathetic of a rabbit that had been subjected to a strong induced current for three minutes, an hour previously, but in which the ear had regained its normal vascularity, immediately produced dilatation of the vessels which rendered their finest ramifications visible for three minutes.

"A strong induced current passing in the same manner through the sympathetic of a rabbit, whether it had been the subject of previous experiment or not, produced during the whole time of its passage (three minutes), a consider-

able shrinking of the vessels; followed, when the current was withdrawn, by an enormous and long persistent dilatation.

"Interrupted galvanic currents passed through the great sympathetic, acted upon the ganglionic vaso-motors in the same manner as induced currents. (pp. 151, 152.)

The conclusions drawn from these experiments are—

"That a continuous constant current, directed upon the exposed great cervical sympathetic of a rabbit, the excitability of which has not been exhausted, (neither by electrization in prior experiments, nor by manipulations, nor by long exposure to the air), produces, like an induced current of equivalent (*i. e.*, very feeble) strength, a slight contraction of the vessels that are controlled by the nerve.

"That the same current acting upon a nerve already exhausted, can no longer produce contraction of vessels, but only moderate dilatation.

"That a powerful induced current, localized in the sympathetic, produces a considerable contraction of the vessels, followed, after the experiment by enormous and protracted dilatation; and that a powerful intermittent galvanic current acts in the same manner.

"Finally, as a corollary to the foregoing, that contrary to certain statements, the continuous constant currents and the induced currents in equivalent doses exert an identical action upon the sympathetic and vaso-motor nerves." (pp. 152, 153.)

This question as to the identical action "of the induced and continuous constant current is another of the disputed questions to which we have previously referred. That there is a difference would, we think, be generally claimed by electricians, while there would probably be quite a diversity in the views entertained as to the essential nature of the difference. Beard and Rockwell regard it as mainly a difference of *degree*.

The section which treats on trophic or nutritive nerves is one of great interest, but must be carefully read to be fully appreciated, and we forbear to enter upon the subject further than to allude to the question raised by Duchenne as to whether certain nervous tubes found in transverse sections of the medulla, cord, and anterior and posterior roots may not be in part the origins of the great sympathetic, or perhaps the dilatator nerves of vessels and the trophic nerves. The tubes present the same diameter as others which have been found in transverse sections of sympathetic ganglia.

In treating of the galvanic current, the author states that at the commencement of his researches, excitation of nerve trunks by continuous currents, seemed to him the method best adapted for the treatment of paralyses. He was much impressed with the experiments of Marianini, Nobili, Matteucci, and others, on animals, but was unable to realize similar results in man. As is well known, he charges Remak with dealing in "theories and hypotheses," and here Ziemssen is of similar opinion, since he says, "We see into what a chaos of hypotheses the dogmas of Remak conduct us."

Duchenne disbelieves in the galvano-tonic reflex contractions which Remak believed to have observed as the result of the passage of the continuous current in patients suffering from hemiplegia, consecutive to cerebral hemorrhage. In one case

"Remak passed the continuous current of from twenty to thirty of Daniell's elements through the right crural nerve, and he saw, after thirty or forty seconds, the segments of the upper limb, and the fingers of the hand of that side which had been strongly flexed, extend themselves completely, while analogous

effects were produced in the lower limb by treating the median nerve in the same manner."

Duchenne repeated Remak's experiments, as he says, "many times." He occasionally observed the contractions, but regarded them as not especially due to galvanism, but as a condition liable to follow most excitants. He says:—

"In one of the patients I several times produced reflex contractions in the paralyzed lower limbs, while causing the passage of a strong, continuous current, now through the median, now through the crural nerve. The leg was extended upon the thigh, the forearm upon the arm, and, at the same time, the hand was opened. The patient, a woman thirty years of age, pointed out to me that the same thing happened nearly always when she yawned, or experienced any emotion, especially upon awaking in the morning. In none of the other patients could I produce reflex contractions, whatever might be the intensity of the continuous current, or of its passage through the nerve. I remarked nothing but contraction of the muscles governed by the nerve excited. The patients, however, told me that their flexed and contracted limbs often became extended, and that their hands would open when they yawned or sneezed, or under the influence of any movement made with effort." (p. 198.)

He also adds, that if this method really cured the paralytic contractions subsequent to cerebral hemorrhage, the theory, as to its therapeutic action would be of little consequence. But since this condition has been shown to depend upon the inflammatory state of the secondary sclerosis of certain nervous bundles, which, when the optic thalami or corpora striata are the seats of hemorrhage, extends to the lateral columns of the opposite side, beginning from the tenth to the fifteenth day, he considers the treatment as of little value. This consideration, however, seems to lose some of its weight by what is said in the context, for after observing as an exception that, in *some cases*, he had observed a diminution of the contractions, and an improvement in the motility by Remak's method, he adds that he had sometimes obtained the same by faradizing the antagonists of the contracted muscles. This, however, is not just the mooted point, and seems to imply a disposition to believe, that the galvanic current may be serviceable in these lesions, if the same efficacy be acknowledged for the induced current.

As has been said, the so-called galvano-tonic reflex contractions, of which the diplegic reflex contractions are a variety, are denied by Duchenne and Ziemssen, and Dr. Tibbits states in addition, that "after the most careful observation, and equally careful and very necessary endeavour to exclude sources of error," diplegic reflex contraction, "so called, has never been observed in the practice and experimental researches at the National Hospital for the Paralyzed and Epileptic."

There are other points in this work of much interest, such as ascending reflex contractions from articular injuries, and the relation of the will to the electric current in the synopsis from Ziemssen, but we will only mention them to lead others to study a work which is replete with original observations and starting-points for further study. With regard to views to which we hesitate to yield an assent, it must be remembered that the conclusions published in this work are evolved from a long experience, and we feel, concerning a doubtful claim,

"Se non è vero è ben trovato."

The work is not arranged for the convenience of readers. It can hardly be called systematic, but no one who wishes to be well informed

upon electricity, and especially the induced current, should be without it. Of course no book of this character is complete without an index, and this is wanting.

Beard and Rockwell's "Practical Treatise" meets very satisfactorily the object for which it was written, and that "is to present in a compact, practical form, all that is now known on the application of electricity to the treatment of disease." It might also be termed a systematic treatise, since the subject is considered under the heads of Electro-physics, Electro-physiology, Electro-therapeutics, and Electro-surgery, thus making the work "more acceptable, both to the majority who seek to consult the distinctively practical portions, and to the few, who may desire also to investigate the subject of electricity in its physical and physiological relations." In addition to these subdivisions which facilitate reference, there is a good index and a glossary of electrical terms.

This volume is one of that useful variety in which practitioners who have given much attention to a particular branch of medical science present in a condensed form the most prominent or reliable views upon it.

The sections on Electro-physics and Electro-physiology are sufficiently full, and will be of much service to the general practitioner as an aid to a fuller understanding of the mechanism of his electrical practice as well as of the general principles upon which the practice depends.

No one ought to be satisfied with merely giving "shocks," and finding that in a certain number of cases such vibratile medicaments were followed by improved health. There must be a good reason for the positive benefits which often follow the employment of electricity, and the more we know of the science the greater likelihood is there of an intelligent application of its principles. The science and the art should be cultivated together.

The section on Electro-therapeutics is well worth reading, and portions of it have already been referred to in this review. The authors dwell in especial detail upon the subject of general electrization, claiming with apparent justice to have first given this method a "distinct and expressive nomenclature," and to have presented the scientific indications for its use; as well as to have properly classified it among remedies.

In the chapter on Galvanization of the Sympathetic, some experiments made by the authors upon the retinal vessels, with the assistance of several other physicians skilled in special departments, are given, and show that the effects of this application are as yet not absolutely settled. We agree with the following sentiment, that "the effects of galvanization of the cervical sympathetic on the retina, like the therapeutical effects, vary more or less with the strength of the current, the length of the application, and the position of the electrodes."

This section occupies the largest part of the work. The different methods of administering electricity are considered and directions given for the application of them. Cases occurring in the Author's practice, some of much interest, are reported with not only the successes but the failures. The view is prominently presented, that medical electricity must be judged like any other remedy from its effects, and clinical experience is held to be the best test for these. Among the interesting topics treated in this section is the reaction of the auditory nerve to the electric current as demonstrated by Brenner. The synopsis here given of Brenner's views will well repay perusal.

The last section, Electro-surgery, treats, as naturally suggests itself, of the application of electricity to surgical diseases. Some parts of this sec-

tion indicate an almost unnecessary detail. What we look for in a work on electricity is not the number of diseases which may be treated by it, but the many ailments in which it seems to act so satisfactorily, when other remedies fail. The cumbrous instruments necessary for galvanic or inductive medication do not come into play if they can be supplanted equally well by simpler agents. Sometimes the latter fail, and then the efficacy of electricity is occasionally brilliant or else produces a sure though more deferred result.

This work is, we think, of very great value for the general practitioner. It is concise and practical, and up to the requirements of the present time in the physical methods of diagnosis. There are misprints here and there which should be corrected, and it would seem as if in a work of so much merit, expressions such as "outsiders," applied to those who are not members of the medical profession, or "taking hold," as applied to the current, might be exchanged for purer and more established English terms. These are perhaps but trifles after all.

There is a freshness and clearness in Dr. Reynold's lectures which makes them very attractive. The volume is an excellent hand-book for ready reference. Some attention is given to the theoretical branch of the subject, but the author evidently regards this as less important than the teachings of clinical experience. In some diseases the current which should be applied is definitely indicated, but in others he uses such language as: "You may divide your cases into two categories; those which will respond more readily to the one form of electricity or to the other; and you are to use in each group of cases that mode of appliance which produces the most distinct contraction with the least amount of electrical force."

As has been stated in a previous part of this paper, Duchenne attributed very different effects to the current proceeding from the coil which first receives the stream from the battery, or the current of the first coil, and that flowing from the second helix. In strong contrast to this is the statement—

"There are two terms used commonly about it, and which you may see on batteries of various kinds—'primary' and 'secondary.' It is for clinical purposes an almost useless distinction. The difference between the two has been said by some to be this—that the primary will have a more distinct action upon one set of nerves, and the secondary upon another. But the most marked physiological difference that can be made out between them is that the secondary is of greater intensity than the primary, and will sometimes proceed more deeply into the parts you wish to affect. It is an unhappy use of the word "primary," for the current so named is not a primary current in the sense of being a simple battery current; it is essentially an induced current, or a battery current strengthened by induction." (*Reynolds*, pp. 12, 13.)

Dr. Reynolds states early in his work, in general terms, what may be expected from electricity. We may often cure or relieve our patients, or we may arrest disease; the treatment has its limits, and "only by its appropriate employment within these limits can it be of real service in either diagnosis or therapeutics." Then follows a description of the different kinds of electricity, and as has been previously observed, the author does not hesitate to recommend franklinic electricity in certain cases. In reply to "what can electricity do?" he first speaks of its chemical effects, as we observe them in the coagulation of blood or the heating of the cauterizing wire, and passes on to the "vital" action of the current on "nerves, muscles, skin, and other tissues." "Vital action" he defines as "certain changes of condition in certain tissues, which we cannot as yet explain by

what we know of the ordinary 'physical' forces, such as the contraction of a muscle, the circulation in a limb, the sensation of pain, and so on."

After dwelling somewhat upon the application of electricity to the different tissues under consideration and making practical points here and there, he proceeds to treat of the diagnostic uses of electricity.

In this section some attention is given to the distinction which should be made between electric irritability and contractility. As he says, these terms "contractility and irritability have been often used to mean the same thing, and are constantly so employed at the present day. This is in my judgment a mistake, for the one refers to the susceptibility of receiving an impression, the other to the capacity for action." The test of irritability in muscle is its comparative readiness to respond to a weak electric stimulus.

"But if we take thirty or forty cells, or a faradization machine, we often find that the same healthy muscle will, perhaps, draw the hand out of the water, while the other (the paralyzed muscle) will merely give a little jerk. The healthy muscle in this case, though less irritable, is more strong. The amount of irritability is in inverse proportion to the degree of electric force which is required to bring it into play." (p. 26.)

We would recommend all practitioners who use electricity to read the sections on diagnosis and therapeutics. The directions for treatment are stated clearly and briefly and as with a conviction arising from practical experience. Probably in consequence of professional interruptions or inadvertence, there is a want of clearness or even an apparent contradiction in the following. Dr. R. says:—

"Another form of electricity—faradization—may also be employed to reduce over-activity. If you find, for example, a man suffering from torticollis—spasmodic wryneck—the sterno-cleido-mastoid and other muscles of one side acting most violently, and turning the head over to the opposite shoulder, you may stop that by passing through the sterno-cleido-mastoid muscle a galvanic current, or by applying weak faradization rapidly interrupted. The interruptions have to be very rapid, for if they are not so, the application only increases the muscular action. * * * * * * Again, in the case of torticollis, where a man's head goes jolting over to one side, you can reduce the over-action by putting the antagonist muscles into action by faradization, and so pulling the head around into its proper position." (p. 59.)

Further it is stated:—

"The forms of spasm in which electricity has been most commonly used, are 'torticollis,' a spasmodic condition of the muscles of the neck on one side; 'writers' cramp,' and so-called 'histrionic spasm of the face.' It is said that such cases have been cured, but my own experience has been unfortunate with regard to them. I have tried electricity again and again, and in every available form, but have never seen it do any good. * * * In torticollis, for the time being, you can put the head straight by either a strong battery current passed through the contracting muscles, or by faradization of the other side, the muscles of which are often weak; but directly you cease the application, the head goes back again into its abnormal position." (p. 96.)

It is easy to understand how such an apparent contradiction might arise as a result of unexpected interruptions during the preparation of manuscript, but it is also evident that the writer has not expressed himself as clearly as he would wish to do.

We consider the section on "The Therapeutical Uses of Electricity," as of great value, particularly to the general practitioner, and would recommend every one to read the third subdivision of that section. It will be

observed that Dr. Reynolds recommends those using faradism to hold the rheophores near together, as in Duchenne's method. This teaching is probably the result of his experience and is a point on which there is a difference of opinion.[1]

In taking leave of Dr. Reynolds's work it is with the intention of keeping it near enough for occasional consultation. It is dense and useful if not voluminous. Certainly, in consideration of the number of readable works constantly issuing from the medical press, it is extremely desirable that authors should condense their writings as much as possible.

Dr. Neftel's work is fragmentary, or, as he states, part of the second volume of his forthcoming work on the galvanic current in its relation to physiology, medicine, and surgery. The author treats of a sublect to which he has given much attention and we shall be very glad to be more familiar with the results of his studies.

There are many points of scientific interest contained in this little volume, and the author evidently writes with a knowledge of his subject. He attaches much importance to the necessity of a complete apparatus for galvanic treatment, and mentions in detail the different instruments necessary to be used in connection with the battery. It is a source of congratulation that at present electrical instruments of the best quality may be obtained in our own country.

We consider the experiment mentioned on page 92, made with the nerve of the galvanoscopic frog, to test the question as to the passage of the galvanic current through the sympathetic nerve, as worthy of repetition and study: also on page 137, a similar experiment with regard to the pneumogastric. With regard to some points which are still undecided we think the author expresses himself, perhaps, too positively.

Still, we will not enter further into the merits of this work at present, which, as previously observed, contains many points of much interest, but wait with eagerness for a fuller exposition of the author's views.

The translation of "Dr. Meyer's excellent treatise" has passed under the eyes of so many reviewers, that we will only call attention to the work as one which should find a place in every collection of volumes upon electro-therapeutics. Although the German nomenclature of electricity is often, as it seems to us, unnecessarily cumbrous, yet no one questions the great value of German labour in the development of electrical science. Dr. Hammond observes that the demand for a second edition has enabled him "to make several corrections in the typography, and to call attention to certain improved forms of electrical apparatus for medical use."

In drawing this imperfect article to a close, we would suggest, with a recent writer on electro-therapeutics,[2] that medical men in describing cases in which electricity has been used, or in writing upon the subject, should employ the terms adapted to the different varieties. To speak of applying faradism or galvanism is a more definite expression than of applying electricity, but even these statements are crude unless the details of application be given. Dr. Radcliffe suggests that

"By applying the method of terminology which Duchenne has so happily

[1] Dr. H. G. Piffard, of New York City, has devised a handle, inclosing two flexible wires, for connection with the positive and negative poles of the induction apparatus. This obviates the necessity of holding the rheophores between the fingers in the somewhat awkward manner recommended by Duchenne.

[2] British and Foreign Medico-Chirurgical Review, July, 1871.

used in respect of the induced current to the other forms of electricity, a series of terms is obtained which would be accurate in form as the practice of nomenclature goes, true to science in fact, free from confusion, and particularly convenient in usage. The series would be (1) *faradaic electricity* or *faradism*; and as respects the pathological and therapeutical application of the agency, *faradization*; (2) *voltaic electricity* or *voltaism*; *voltaization*; (3) *franklinic electricity* or *franklinism*; *franklinization*."[1]

The study of a number of works, simultaneously, upon the subject of electricity, renders it evident that very much still remains to be accomplished in this field. We may picture to our minds a young practitioner, anxious to bestow especial attention to electricity, with reference to the practical employment of it. He would, as a preliminary, endeavour to render himself familiar with the investigations of others upon the subject, and the conclusions to which they had been led. While he would find that on some few points there was a similarity of opinion, on others he would observe a wide variance, and might come to the conviction that this variance depended occasionally on the circumstance that one writer was of sanguine, and another of doubting mood. Thus, "in wandering mazes lost," if not discouraged, he might, while retaining the general principles of the science, turn to the practical application of the art, and here he would very likely meet with a success in some cases absolutely brilliant—a success which would cause him hardly to wonder that Watson's cures by this agent in 1763, were confounded with witchcraft. Certainly the action of electricity, as a therapeutic agent, is in some cases exceptional and most satisfactory.

The future history of medical electricity is somewhat uncertain. Judging by the past, it will be progressive, and will continue to hold a firm place in therapeutics. There are certain results, such as the disintegration of calculi, the cure of aneurisms, the destruction of morbid growths, which have been cherished hopes, but which still remain "so near and yet so far." Knowing that the study of electrical phenomena occupies some of the best medical minds, that a retrospect shows a decided advance in the field; that notwithstanding the doubts and discouragements which have attended experimenters, there is an increased confidence in this agent, we have good reason to believe that the best results of electro-therapeutics are yet to be attained.

F. A. B.

ART. XV.—*Third Annual Report of the State Board of Health of Massachusetts.* January, 1872. pp. 329. Boston, 1872.

NINE months ago we noticed at some length the Report immediately preceding the one above named, and commented upon the remarkably high character of the Board. Now, as then, it might safely be said that better appointments could hardly have been made by any scientific or philanthropic body than were here made by the State legislature. Only one member out of the seven composing the Board has been changed. Dr. Bowditch and Dr. Derby, as chairman and as secretary, continue in the offices they have so worthily magnified.

[1] Practitioner, vol. i. p. 19, quoted in Dr. Tibbit's Translation of Duchenne.

We sketched, in the article referred to, the general plan pursued by the Board in collecting information. The system has been continued of appointing prominent physicians in all the towns, as sanitary correspondents. These, as heretofore, have not only presented general health reports, but have replied to particular queries addressed them by the secretary concerning special subjects of investigation. When any particular line of inquiry has seemed to require the skill of an expert, to elucidate the facts and draw from them the legitimate conclusions, the Board seem to have selected the men best fitted for the work.

The expenses of the Board, though five hundred dollars greater than for the preceding year, are still wonderfully small. Out of the standing appropriation of $5000, only $2850 has been expended. We believe the intelligent people of the old Bay State will feel that the noble volume of essays before us is alone worth much more than the entire expense. Indeed if the cost of this report be compared with that of many other State, municipal, and national documents, some startling contrasts may be found.

The general report on the "*Health of Towns*," made up principally from the correspondence received, contains few matters requiring especial notice. Additional facts of interest continue to be presented as to the connection between typhoid fever and local conditions, concerning which, it may be remembered a large and curious mass of information was given in the second report.

A very interesting account is given of extensive and unaccountable lead-poisoning occurring among the operatives in a gingham factory. It was only after several weeks of research that the source of the symptoms was discovered in a yarn dyed with chromate of lead. A needless amount of the pigment was used, in this instance; and the weavers habitually threaded their shuttles by sucking the yarn through the eye of the shuttle with their mouths. A hook was devised and ordered to be used for threading; and the dyeing ordered to be more carefully conducted. The epidemic speedily ceased. This gratifying instance of the triumph of medical science is creditable alike to the physician who made the investigation and to the Board through whose action such researches are undertaken, or encouraged, and made to contribute to the health and the enlightenment of the human race.

Another essay which may be regarded as a continuation or sequel of a paper in the second report is the one entitled "*Analysis of the Correspondence on the Use and Abuse of Intoxicating Drinks throughout the Globe, . . . or Intemperance as seen in the Light of Cosmic Law.*" The earlier paper consisted of a voluminous correspondence, with a brief summary of its teachings. The present essay, while giving some additional letters from foreign lands, consists principally of an elaborate presentation of the generalizations and inferences which Dr. Bowditch thinks legitimately derivable from the facts drawn from all races, climates, and conditions. Dr. Bowditch begins by stating that the use of stimulants is due to a natural and universal instinct. This once admitted, he argues that it is both idle and wrong to attempt to eradicate the craving. Like all natural tendencies it is implanted for wise purposes and is to be controlled, not exterminated. Attempts in the latter direction he thinks full of harm.

Taking up the results derived from his correspondents he proposes to consider intemperance in relation to isothermal lines, to race, to the kind of stimulant used, and to the prevalence or absence of grape-culture.

While these divisions of the subject are not very closely followed, they yet give an idea of the direction and tendency of the doctor's views. In warm countries he finds intemperance almost unknown, except among foreigners who may have brought with them the appetites of other races and other climates. In the high latitudes it is that intemperance becomes so terrible a curse.

Other things being equal, a country in which the grape thrives exhibits much less intemperance than one where it does not flourish.

National habits and customs, and to some extent legislation, have also important relations to temperance. The existence of open bars, the habit of "treating," and heavy duties on the pure light wines of France and Germany, are regarded as encouraging intemperance.

Progress in the temperance reform in our land is to be looked for, in the opinion of our writer, not from prohibitory legislation, but from extended cultivation of the grape, in popularizing the German beers, and German methods of drinking. For the open bars at which men pour down deep draughts of fiery and often poisonous spirits, he would strive to substitute the "Garden" or "Hall," where men sit with their wives and children and slowly sip a beverage scarcely so potent as tea.

Much may be done, Dr. B. thinks, in the way of creating a more wholesome public sentiment. Let the young be impressed with the degrading character of excess, rather than with the idea that all stimulants are necessarily evil. Strive to build up a distaste for the two prevalent American customs of "treating" and drinking at bars. In the training of children Dr. Bowditch regards two courses as almost equally evil—one by which without caution, or guidance, or control, they are allowed to acquire the habitual use of stimulants; and the other in which they are taught to look upon all stimulants as equally, wholly, and necessarily bad. In the latter case, when home restraints are removed, a dangerous reaction of feeling too often occurs; for, finding that facts have been misrepresented and that an innocent pleasure has been denied, young men may rush into the very excesses which their training was intended to render impossible. The greatest safety, he thinks, is to be found in a middle course.

It requires some boldness, at this day and in Massachusetts, to uphold views such as these. No one who knows the author of this paper, however, will for a moment doubt that his views are as sincerely held as they are vigorously expressed. Whether or not his ideas are all judicious, it is certain that the advocates of different views have shown so much bigotry and intolerance of opposition that it is well to have these more moderate opinions, and perhaps more practicable methods, so fully and ably set forth.

"*Arsenic in certain Green Colours*," by Dr. F. W. Draper, is a paper of great value, prepared with much care and research. Two pigments in common use are named, as containing poisonous amounts of this drug. These are, Scheele's green, made of arsenite of copper; and Schweinfürt green, made of aceto-arsenite of copper. Practically, the two are often confounded, and called by each other's, or a common, name. The first contains 55 per cent., and the second 58 per cent., by weight, of arsenious acid, or "white arsenic." Beauty of tints, with ease and cheapness of production, have rendered this colour very popular. One English maker of paper-hangings stated, in 1860, that he used two tons of arsenic weekly. Dr. Draper goes on to point out the forms in which this deadly poison

enters our homes and our workshops, and the evil effects resulting, often wholly without suspicion of their true cause.

In making the green leaves of the artificial flowers worn by ladies, the arsenical pigment is made into a paste with water and starch or gum arabic. This is spread upon, and rubbed into, cloth, much as the surgeon applies plaster of Paris to his moistened rollers. The more the cloth and pigment are manipulated the better the colour adheres. No mordant is used, to fix, by chemical action, the colour in the fibres of the cloth. The union is simply mechanical, as in the case of the plaster and muslin. When the material is dry, it is cut, stamped, moulded, and formed into the verdure that so beautifully imitates the natural setting of buds and flowers. In these processes much of the colour comes off as a fine dust. Still worse, in some cases the leaves are dipped in melted wax and the powdered pigment sprinkled on them from a dredging box. Of course, during this process the air becomes filled with floating atoms of deadly poison. While it may well be believed that the workers upon these dangerous ornaments are the persons most liable to suffer, we can well understand that dealers and even wearers may not be wholly safe. Investigations in England, France, and Germany reveal most deplorable facts as to the debility, nausea, diarrhœa, eruptions, ulcers, inflamed eyes, and other affections prevailing among the men and women engaged in this manufacture. Death has in some cases directly resulted. That it has not oftener followed is due to the usually gradual character of the arsenical poisoning, which obliges the sufferer to give up work, and remove from the cause of disease, before the recuperative forces are exhausted.

Green tarlatan, likewise owes its colour to the mechanical presence of the same deadly dye, insecurely fixed by starch or sizing. Some specimens have been found to contain nearly one-half their weight of pigment. A sample analyzed by Prof. Nichols, of the Mass. Institute of Technology, gave 8.21 grains of arsenious acid to the square foot, corresponding to three or four ounces, in a ball-dress. One dress which seriously poisoned its maker, in France, was found to hold eight ounces. Many instances are recorded where illness was traceable to working upon this fabric. The essayist suggests, too, that a portion of the responsibility for the notorious feeble health of dressmakers may very possibly rest upon these green materials.

There are comparatively few records of illness caused by merely wearing green leaves and tarlatans. These, too, may have been due to unusual susceptibility to the poison. But even if this be the fact, it should be gravely considered whether it be right in any way to encourage the manufacture of adornments so injurious to the makers.

One fact of importance to parents is that the wrappers of bonbons, and in rarer cases the confections themselves, are stained with the arsenical green. Green-painted toys, and the cakes of green in boxes of toy water-colours, are also very poisonous. It would seem proper that our local boards of health should inspect toys and sweetmeats with a view to preventing the use of dangerous colouring matters.

Green paint on our inner walls ought also to be carefully avoided, unless known to be non-arsenical.

Green papers are to be avoided or used with caution, unless we have ascertained their colour to be due to a harmless dye. Tickets, cards, and fancy papers are often picked up by young children and put at once to their lips. Wall-papers, like tarlatans, contain often very large amounts of arsenical

green, very loosely held between and upon the fibres. Especially those with a rough, unglazed surface, part with their colour very easily. The essayist states a large number of instances where the most serious illness was clearly due to green wall-papers. The cause not being at first suspected, in many cases the victims, convalescent after removal for change of air, or after confinement in a non-poisonous bedroom, have suffered relapse on returning to the green-papered office or sitting-room. The cases given are very numerous, fully reported, and perfectly conclusive in proof of the frequency of serious disease resulting from green paper-hangings.

From many of these arsenical papers the colour easily comes off on the hands and the clothes. After brushing the walls it may be seen in the form of green dust lying on the furniture. A certain paper which had been on the walls for four years, and which had proved very injurious to health, was on that account removed. A certain measured portion being analyzed indicated the presence on the four walls of the room of no less than one-half a pound of white arsenic. Workmen applying and removing such papers have often suffered severe, though transient, illness.

Interleaved with the pages of this essay Dr. Draper shows us some specimens of arsenical papers. A glazed sample is said to contain 8.67 grains of arsenic to each square foot. One unglazed sample, having a tint so delicate as hardly at first to be recoguized as green, gives 5.42 grains; another, a rich bright emerald, 29.32 grains.

Dr. Draper justly remarks that the importance of the facts and cases given is not affected by the possibility of some men living for years apparently unharmed by exposure to similar influence. Were extreme tolerance the rule, and such harmful effects as he narrates only the exception, he would still have shown the use of such wall-papers to be dangerous and reprehensible.

A valuable article by the secretary, Dr. Derby, treats of "*Mill Dams and other Water Obstructions*," as affecting the public health. In the earlier years of the New England colonies the natural falls of hundreds of streams were used to move the village grist-mills. As manufactures multiply and water-power is more and more in demand, dams are built to heighten the fall. Of course this, to a variable extent, interferes with the drainage of the country above. A nearly level tract, formerly dry, may be converted into a swamp. The flow of the stream becomes sluggish. To provide against the long summer droughts, great ponds or reservoirs are formed in which water stagnates, and into which probably is thrown or poured all manner of filth from the houses of the operatives. When, under the burning suns of August and September, the natural flow ceases, the reservoir is drawn upon, and partly or wholly emptied. The morbific influence of the drying of such a surface in hot weather scarcely needs statement. In the elaborate paper on typhoid fever, forming so important a portion of the second report, noticed in our October number, it is stated that precisely this cause was assigned for local epidemics of the disease, in twenty-one out of one hundred and sixty-three towns, far exceeding any other single cause. It is recorded that in 1828 a dam was built across the Housatonic River, two miles south of the line between Massachusetts and Connecticut. This, affecting the height of water for ten miles above, was followed by so much intermittent that the people obtained legal order for its removal as a nuisance.

In this connection Dr. Derby expresses the conviction that sporadic in-

termittent is not now, as twenty-five years ago, wholly confined, in New England, to the Housatonic and southern Connecticut valleys. He, moreover, remarks that typhoid often seems modified by an intermittent tendency, and that quinia is becoming more and more useful as a febrifuge. As a confirmation both of the increase of intermittent and its connection with dams and mill-ponds, some striking facts are given concerning the recent epidemics in the neighborhood of New Haven.

Dr. Derby very forcibly exhibits the evils of tide-dams in and near large towns. These, intended to utilize as power the ebb and flow of the tide, through inlets and estuaries, cause the permanent retention of masses of water in places which otherwise would be completely emptied and filled twice daily. The basin thus formed, if near a city, receives sewage and becomes a foul and putrid sink. Many years ago large numbers of these tide-dams, with their mills, were built along the Massachusetts coast. Wherever these have become surrounded by a dense population, they have become offensive and dangerous nuisances.

A brief paper on "*Adulterations and Impurities of Food*," by Henry B. Hill, gives these results: Fruits preserved in tin cans, though presenting traces of lead, copper, and tin, are practically harmless. Of vinegars, those sold as cider vinegars are almost always genuine, though sometimes watered, while the wine vinegars contain frequently more or less free sulphuric acid. Two samples were found to contain lead, one in terribly large amount, one-half of one per cent., equivalent to four and one-half grains of sugar of lead to the fluidounce. In these the poison was derived from leaden faucets, most wrongfully employed. Of seven samples of so-called coffee, sold in pound packages ready for use, one contained no coffee whatever. Four contained from fifty to seventy per cent. Peas, beans, and grain, chiefly wheat, entered largely, and chiccory was found in two packages.

A report is presented by Dr. F. E. Oliver, of Boston, upon the "*Use and Abuse of Opium.*" The paper is intended to direct attention to the employment of this drug as a stimulant and intoxicant. Two questions were addressed to the medical correspondents of the Board in the various towns of the State. The first as to the use of the drug for other purposes than relief of pain; the second, as to whether or not such use were increasing. Two-thirds of the one hundred and twenty-five physicians answering, report a greater or less amount of illegitimate consumption of opium in their towns. Many druggists, also, in Boston and in smaller places, state that large quantities of the narcotic are used without prescription. From the opinions of prominent importers and druggists in New York and Boston, Dr. Oliver estimates that at least one-half the opium imported is used as a stimulant. This estimate, moreover, reckons the amount used in quack remedies with that otherwise prescribed; and excludes from consideration the drug prepared for smoking. As to the extent to which the "opium habit" prevails, the information is, as might be expected, indefinite and various. Certainly its prevalence is sufficient to require the practitioner to remember its symptoms when he encounters forms of impaired health for which he cannot at once account.

As to the increase of this illicit consumption, the indications derived from custom-house returns are very startling. In view of the great increase in the use of milder and newer anodynes by physicians, and a clearer perception of the dangers of even a medicinal use of opium, we believe we are warranted in assuming that the legitimate consumption of the

drug has not of late increased out of proportion to population. From 24,000 pounds in 1840, we have risen to an importation of 154,841 pounds in 1870. The writer goes on to state that the amount for the first half of 1871, is 123,000; and hence estimates 246,000 pounds for the entire year. The increase from 1840 to 1870 is sufficiently alarming; but that from 1870 to 1871 is, if correctly estimated, truly terrible. May it not be possible, however, that the arrivals of opium occur principally in the first six months? Dr. Oliver does not state whether he inquired as to this point. At all events, if our assumption be correct, that physicians are not prescribing more and more opium year by year, there must be a most serious extension of the "opium habit." No doubt it is true, as intimated by Mr. Hayes, Mass. State Assayer, that of late years opium enters more largely than ever into proprietary and family medicines, "cough mixtures, lotions, liniments," etc. It has been found too, he tells us, in liquors. But practically and in effect, this sort of use belongs rather to the illegitimate than to the proper medicinal use. One of the most diabolical manifestations of the sordid and heartless quackery which thrives in this age, is seen in the fact stated by Mr. Hayes, that some of the advertised "cures" for opium-eating contain potent doses of morphia. One of these, if taken as directed, would give six grains of sulphate of morphia daily. Truly the last state of the unfortunate believer in this "cure" would be worse than the first! Dr. Oliver directs attention to the abominable "cordials," "cholera cures," and "soothing-syrups," as not only destructive of infant life, but as very probably sowing the seeds of an irresistible craving for stimulants in after life. When we learn that a recent analysis of "Winslow's Soothing Syrup" gave one grain of morphia to the fluidounce, we are prepared to expect any amount and degree of cerebral or nervous disorder in the adult life of any child who might outlive its frequent administration.

Dr. Arthur H. Nichols contributes a very full and thorough consideration of the "*Effects on Health of the Use of Sewing-machines moved by Foot-power.*" Not long after the sewing-machine came into common use, rumours began to be current concerning its ill effects on the health of its workers. A number of physicians acting independently, and after investigations of varying extent and thoroughness, joined in the popular opinion, especially laying to the charge of the machine the production or aggravation of the diseases peculiar to women. Some went so far as to assert that paralytic and peculiar nervous lesions sometimes resulted. Dr. Nichols has very fully and carefully investigated the whole subject, both through questions addressed to the sanitary correspondents, and through an extended personal visitation and inquisition. His researches clearly indicate that continuous labour at machines moved by the ordinary treadle is too severe to be long pursued by women of delicate organization, without impairment of health. Such women, labouring often in close and crowded work-rooms, are liable to become the subjects of general debility. This, eventually, may manifest itself in many ways. But, moreover, there seems now to be no doubt of the fact that over-work at the sewing-machine treadle is peculiarly liable to cause hyperæmia, or congestion, of the pelvic organs. This, when we consider the constant action of the muscles connecting the thighs with the pelvis, especially the psoæ muscles, combined with a stooping posture, and perhaps tight corsets or waistbands, is not at all strange.

While what we have just stated seems fully supported by the evidence,

it is equally true that in some work-rooms, exceptionally well ventilated, and where the peculiar character of the work allows frequent pauses in the motion, evil results may be hardly noticeable. Women of strong constitutions, too, and not disposed to uterine disease, may labour for years, even amid unfavourable surroundings, without apparent injury.

As to the use of the foot-moved machine in private families, where other household avocations occupy the greater part of the time, as a rule it may be confidently asserted that no ill results follow. The average American woman may safely use her machine at odd minutes or hours, for one, two, or three hours daily. Nevertheless, in cases of known weakness of the pelvic viscera, it will be the part of prudence to inquire pretty closely as to any connection between the machine and the occasional exacerbations of the malady.

The excessive severity of the labour required to run sewing-machines by foot-power, ten hours daily, is sufficiently well established to call for some remedy. Dr. Nichols, therefore, points out instances which prove that in large establishments, the introduction of steam-power is not only humane, but economical. Each worker, when assisted by steam-power, can do much more work daily, and preserve much greater health and strength. Where the operative works at home, or where steam-power is inaccessible, two improved treadles, the "Parsons" and the "Hall," are very highly commended. If it be true, as indicated by the few experiments given, that these so greatly lighten the labour as to wholly prevent the exhaustion caused by the old treadle, public opinion should demand their introduction into all workshops where foot-power is used, and upon all newly-built machines.

One of the first objects proposed to itself by the Board, on its organization in 1869, was the regulation and reformation of *slaughtering, and the processes connected with it.* The present report gives a full account of the investigations made, reforms attempted and results gained. In the report published in 1870, the unwholesome, wasteful, and every way objectionable character of existing slaughter-houses and their adjuncts, was clearly set forth. Large abattoirs were urgently recommended, which should include all improved facilities for utilizing offal in such manner as to prevent, or totally consume, all offensive vapours. The possibility of attaining the end desired was clearly shown, with the added advantage of important revenue to be derived from matters hitherto wasted. But, as is often the case with proposed reforms, the men who were to be most directly benefited were the most bitter in opposing any change. The killing for the Boston market was almost wholly done in private establishments clustered together in the suburban town of Brighton. Here population was largely increasing, and room was desired for residences for men doing business in the city. The cattle-pens, slaughter-houses and fat-rendering establishments, never scientifically constructed or managed, were becoming more and more intolerably offensive. Feeling the urgent need of speedy relief, the Board obtained the passage of an act authorizing the incorporation of a stock company which should have the right to build grand abattoirs, provided with all appliances for securing the best results, as to excellence of meat, utilization of refuse, humanity, and economy. These were to be constructed according to plans of the Board, and to be grouped together very near the site of the existing slaughter-houses. Seeking in every way to conciliate, and to remove all reasonable grounds of opposition, the Board hoped for the cordial coöperation of the butchers. Prejudice, how-

ever, was too strong; the butchers would not take stock, and the company could not organize. At the next session of the legislature these facts were stated and further legislation asked. The importance of the subject was recognized and an additional act passed. This empowered the Board to close all establishments which their judgment deemed inimical to "public health, or public comfort and convenience." Supported as it was by a rapidly strengthening public opinion, this act brought the butchers to their knees. They humbly begged to be allowed to remain in their old houses, pledging themselves to make all changes and improvements that the Board should suggest. Convinced of the necessity of a radical change, the Board firmly refused. Complaints were very soon made, and in Brighton alone, under the new law, some six or eight houses were closed by the Board. This firmness soon bore fruit. Conferences were held, and very soon the butchers were organized as a joint-stock company in the manner and under the restrictions prescribed by the original act. A large tract of land, between and closely adjoining the two great cattle-bearing railways, on the Charles River, and perfectly adapted to its object, is already purchased. Upon this will be erected buildings for the reception, feeding, killing and dressing of all cattle, sheep, and pigs needed to supply the city markets; and also all the varied establishments for preparing the hides, the fat, the blood, and all products whatsoever. Nothing will be wasted. It is believed that the entire system can and will be made entirely inoffensive. All will be constructed, and afterwards conducted, under the authoritative supervision of the Board. A rigid inspection will follow each beast from his arrival in the cattle-train till his dressed meat is exposed in the stalls and his other portions converted into valuable and innocuous manufactured products. Cruelty will be prevented; the consumers will be insured wholesome meat; and adjoining residents will be protected to the fullest possible extent from all the annoyances which have usually surrounded the slaughter-house. To this great central abattoir the Board have power to compel the removal of any person carrying on the slaughtering or allied trades within six miles of Faneuil Hall, if in their judgment the public good require it.

It would take too much space to set forth the improved processes by which the Board confidently expects to destroy bad odours, and to make from products formerly nearly valueless, a most valuable fertilizer. Suffice it to say that all the details seem to have been thoroughly worked out and arranged.

Thus bravely fought and nobly won was the battle of the Board against the butchers. Of its hard-earned victory it may well be proud. As to the people of Boston and its neighbourhood, those who know the terrible amount of unwholesome meat formerly sold, will realize that they have reason to be grateful for blessings even more substantial than relief from bad smells.

Dr. James C. White, who, though still a young man, is prominent as a dermatologist, contributes an elaborate article of some fifty pages upon "*Vegetable Parasites, and the Diseases caused by their Growth upon Man.*" After stating the fungous nature of these organisms, and alluding to the possibility of confounding their accidental with their causative presence, Dr. White names three common forms or types of skin disease which are undoubtedly produced by the action of the three fungous growths which accompany them. These are, favus, herpes tonsurans [in reality including all the various forms of ringworm], and pityriasis versicolor.

Each of these, our writer claims, is a perfectly distinct disease, having each its peculiar parasitic cause.

Under the second of these heads Dr. White describes a number of differently named eruptions which he believes to be identical in essence, and caused by one and the same fungus. Herpes tonsurans, tinea tonsurans, herpes circinatus, sycosis, eczema marginatum, "and the majority of the cases of parasitic disease of the nails, called onychomykosis, are all caused by the growth of one and the same fungus, hitherto generally entitled trichophyton tonsurans." The dissimilarity in the appearance of some of these forms of disease he attributes to the site and to the stage of growth. In regard to sycosis or mentagra, Dr. White points out the additional and important fact, that while the outward manifestations to which these names are applied are usually the result of aggravated and chronic ringworm, as proved by its being sometimes observed to commence like simple ringworm, and by its containing the same fungus, and by its giving rise, by inoculation, to simple ringworm, yet nevertheless very similar appearances occasionally result from a simple follicular inflammation wholly free from fungous agency. He believes also that the contradictory evidence of competent observers as to the etiology of alopecia areata is explainable in the same manner, or that one class of cases depends on simple atrophy of the hair-bulbs, and another on the presence of a parasitic fungus.

Dr. White makes a very forcible and conclusive argument for the specific and individual character of the favus fungus, against those writers who have striven to prove it to be only a development of the one causing ringworm, and against those who believe both growths to be but modifications of common "mould." The weight of reason and evidence, it seems to us, is strongly on his side.

Attention is directed to the frequency with which favus and ringworm may be observed on the lower animals, containing fungi identical with those which afflict the human subject. The doctor saw some reason to conjecture that the common mouse may be the original victim of the favus fungus and the means of its communication to man. Cats and dogs have been observed to contract the disease from diseased mice.

This very able and instructive paper closes with some practical remarks as to the modes of contracting, and of avoiding, these disgusting affections. The chief lesson drawn from Dr. W.'s knowledge and experience is that we should carefully avoid the barber's shop. Keeping private mugs, soap, brushes, and razors is not a trustworthy safeguard. The operator's hands, his strop, or possibly his aprons or towels, may convey the contagion. Dr. White has several times observed ringworm on the hands of barbers.

This essay is full of interesting observations and thorough discussions, much of which we have not even hinted at. The paper is well worthy to be published in a separate form as a permanent and valuable contribution to the literature of dermatology.

A brief essay on "*Smallpox*" in Massachusetts presents nothing worthy of especial note. Until 1870 the deaths from this disease had been below the average of former years. In 1870, 131 deaths occurred, nearly one-fourth in Holyoke. The number for 1871 had not yet been reported, but was undoubtedly larger than before, and principally in a very few towns. Circulars were sent out to all the towns, urging the necessity of thorough vaccination and revaccination, and asking information upon the subject.

The Board recommend that an existing law, requiring vaccination before two years of age, be amended to read, before six months. Also that local health officers have full power to remove and isolate cases of variola. The correspondence would seem to indicate that, as in other States, vaccination had been culpably neglected.

An essay on the "*Proper Provision for the Insane*" is contributed by Dr. Edward Jarvis. This gentleman is acknowledged to be the ablest statistician in the United States. Better than any other he has learned to distinguish between what figures really prove, and what they may be made to seem to prove. He has been interested in the study of insanity for many years. His practical familiarity with the disease, however, has not been derived from constant intercourse with the varied types to be found in the crowded wards of large hospitals; but rather from the small number of selected cases who have resided as invalid boarders in his private house.

It is a little difficult to understand the drift of the present article. In the outset Dr. Jarvis enlarges on the partial character of many cases of insanity, which he compares to broken limbs, or derangements of special organs, leaving the rest of the body unaffected. Only the parts which are diseased, he says, require to be rested or restrained. Existing hospitals for the insane, he intimates, carry restraint or forced rest, too far. He seems to think that by some changes in their structure and management the degree of restraint can be more nicely graduated and better adapted to the exact needs of particular cases. A number of cases are described in which patients afflicted with "mild" or "partial" insanity have recovered promptly on being sent to visit friends, or to board with judicious strangers, or even at home under a careful adaptation of their surroundings to their peculiar needs. Still, he frankly admits that for the vast majority, even of "mild" cases, the hospital is the only means of cure practically available. Only the exceptionally wealthy, or those having even more exceptionally discreet and self-sacrificing friends, can expect to find in private families the curative influences required. After some further expansion of his somewhat vague objections against the existing hospitals—which, yet, he acknowledges have done and are doing a noble work—he proposes as a remedy for all faults, the adoption of the "American cottage plan." This, as some of our readers will remember, consists of an ordinary hospital as a centre, having connected with it various detached buildings constructed like private dwellings. How it is thus expected to secure to poor and even pauper insane advantages which otherwise can scarcely be attained even by the most wealthy, we do not comprehend. In an article in the April number of this Journal we fully stated what seemed to us to be insuperable objections to this much-vaunted plan. Fully agreeing with Dr. Jarvis that there is room for improvement in our hospitals, we do not believe it is to be sought for in the direction which he has somewhat indistinctly indicated. Certainly, in the present state of public opinion, we cannot approve of any new departure in hospital construction which will increase the cost of these institutions. B. L. R.

ART. XVI.—"*Hospitalism.*"

1. *The Works of Sir James Y. Simpson, Bart.* Vol. II. New York: D. Appleton & Co., 1872.
2. *A System of Surgery.* Edited by T. HOLMES, M.A., Cantab., 2d edition. Vol. V. New York: Wm. Wood & Co., 1871.
3. *Medico-Chirurgical Transactions.* Vol. XLII. London: 1859.
4. *The Same.* Vol. XLVII. London: 1864.
5. *Guy's Hospital Reports,* 2d series. Vol. I. London: 1843.
6. *The Same,* 3d series. Vol. XV. London: 1870.
7. *Saint Bartholomew's Hospital Reports.* Vol. V. London: 1869.
8. *Saint George's Hospital Reports.* Vol. I. London: 1866.
9. *The Lancet.* Vol. II. London: 1869.
10. *The Boston Medical and Surgical Journal,* new series. Vol. IX. Boston: 1872.

THE ravages of Hospital Gangrene in the hospitals of France, during the last century, prompted Pouteau to question whether or not those institutions of mercy were actually more pernicious or more useful to society; and a similar query, propounded a few years since by the late Sir James Y. Simpson, has given rise (as our readers doubtless know) to a controversy only surpassed in acrimony by the celebrated "Acupressure Controversy" which was originated by the same eminent obstetrician and author, who, in addition to all his other talents, certainly possessed, in an eminent degree, that of exciting the indignant opposition of those surgeons and surgical writers with whom he came in contact.

Now that the grave has closed over the body of the illustrious author who started the discussion concerning "hospitalism," and now that the winged words by which he being dead yet speaks are reaching, in the goodly volumes which constitute his collected works, a much larger audience than listened to his living words at that celebrated Leeds meeting of the British Medical Association, it seems an appropriate time to take, as far as may be, a calm survey of the field of conflict, and endeavour to answer without prejudice and without partiality Sir James Simpson's certainly momentous inquiry, "To what extent are hospitals, as at present constituted, banes or blessings? and [if the former] how can they be changed so as to convert them from the former to the latter?"

In attempting to answer this question, we shall chiefly confine ourselves to the function of hospitals specially animadverted upon by Sir James, namely, as places in which to have performed important surgical operations, and we shall use as a framework for our remarks the various "Propositions" which were advanced by the Edinburgh professor in his reply to Mr. Holmes, and which in the collected edition of his works are reprinted as chapters III. and IV. of the papers on "Hospitalism."

Sir James Simpson's first proposition is that "the aggregation or isolation of patients regulates, in a marked degree, the results of operations, etc., upon them." Now, if this be taken to mean simply that the overcrowding of patients exercises an unfavorable influence on the results of operations, probably all would agree that this proposition should be affirmed: it is a matter of universal experience that when a hospital or a ward is constantly forced to receive more patients than it has normally accommodation for, the health of its inmates will suffer, and the results of operations will probably be less favourable than when it is not crowded

beyond its normal capacity. But this is not the sense in which Sir James Simpson meant his proposition to be taken: he maintained—and attempted to prove by statistics—that the size of a hospital was *per se* a determining cause of mortality, and that the death rate after amputation was directly proportionate to the number of patients harboured in the same building, no matter what its hygienic arrangements. Seven thousand cases of amputation which he had collected (two thousand from private country practice, as many from the records of metropolitan hospitals, and about three thousand from those of provincial hospitals), showed to his own satisfaction, though not to the satisfaction of those who differed from him, that the mortality after amputations in the continuity of the limbs was,

"1. In hospitals from 300 beds to 600, ... 1 in 2½
2. In hospitals from 100 beds to 300, ... 1 in 4
3. In hospitals from 25 beds to 100, ... 1 in 5½
4. In cottage hospitals under 25 beds, ... 1 in 7
5. In isolated rooms in country practice, ... 1 in 9."

With regard to the mortality after amputation in large hospitals, it has been pointed out by Mr. Holmes, Mr. Callender, Mr. Birkett, and others, that the high death rate can be readily, and we may add most reasonably, accounted for in other ways than by attributing it merely to the size of the hospital, and with regard to the mortality "in isolated rooms in country practice" the criticisms of Messrs. Holmes and Callender, as to the true signification of Sir James Simpson's statistics, seem to us eminently just. The death rate of 1 in 9 (really 1 in 9.2), is derived from an analysis of 374 returns from private practitioners, these returns embracing in all 2098 cases. But, as pointed out by Mr. Callender, 217 of these returns (embracing 779 cases) do not include a single death; 62 of the operators having each amputated but once, 59 only twice, and the average number of operations performed by each of the 217 surgeons being but 3½: as Sir James Simpson acknowledges, some of the blanks which he sent out were not returned at all, and it is, as remarked by Mr. Callender, altogether likely that some of these blanks may have come into the hands of operators whose experience had been as exceptionally unfortunate as that of the happy 217 had been fortunate. Mr. Holmes referring to the same point says:—

"My conviction from reading Sir J. Simpson's figures is that his list is composed of returns from surgeons, who, having been gratified by their success in the emergencies of practice, have treasured up records of that success, and have been glad to communicate it. Nothing can be more natural, nothing more legitimate than this. If, like the surgeons marked No. 92 and No. 171[1] in Sir J. Simpson's list, I had amputated for injury 22 times; or like No. 191, 29 times; or like No. 288, 52 times, without losing a single patient, I should be most happy to take any proper opportunity to say as much, and more particularly if the question was asked; but if, on the contrary, I had (as I know many surgeons in the country have), had one or two unsuccessful cases of amputation in a long experience, chiefly occupied in other branches of practice, and in which death occurred from previous disease or concomitant injury, or operative disaster, or the want of proper attendance, why should I keep any record of facts which really prove nothing? or why should I make any return of them in a sanitary inquiry?"

To this criticism, Sir James replies that these four surgeons were less successful in their pathological amputations; that four other surgeons,

[1] Return No. 171, as pointed out by Sir J. Simpson in his reply to Mr. Holmes, really gives 2 deaths to 22 amputations.

whose returns are embraced in his tables, lost 25 out of 128 cases, or about one in five; and that in eight or nine of his returns (Mr. Callender gives the number as 12), the amputations are reported as "uniformly fatal." The last mentioned returns certainly speak well for the honesty of the reporters, but, we cannot help asking, is not this 8 or 9, or even 12, a beggarly corporal's guard in comparison with the noble company of 217, whose operations were "uniformly successful?"

We confess that we are disposed, with Mr. Callender, to take the unknown number of Sir James Simpson's blanks, which were not returned at all, as in some degree a set-off to the returns which embraced only successful cases, and perhaps the fairest way of approximatively estimating, from the imperfect figures furnished, the true death rate of amputations in country practice, would be to reject the returns both of uniformly successful and of uniformly fatal cases; this would leave 145 returns, embracing 1305 amputations, with 212 deaths, and would give, therefore, a mortality of nearly one in six, or 16.25 per cent. These figures, as will be seen hereafter, do not so very much differ from those derived from the records of country or provincial hospitals, no matter what their size.

Passing on to Proposition II., in connection with which we will also consider Proposition XI., we are told (in the former) that the differences in the death rates of different classes of hospitals, and (in the latter) that the differences between the death rates after limb-amputation in country practice and in hospital practice are "surely not to be explained by differences either in the constitution of those operated upon, or in the nature of their injuries or diseases." This assertion is controverted, and we think most successfully, by Sir James Simpson's opponents, and indeed is contradicted by the experience of certainly the very large majority of practical surgeons. Taking the first branch of the proposition, we do not hesitate to say that "differences in the constitution of those operated upon" exercise a most important influence on the results of the operations performed. Nearly thirty years ago, Dr. Norman Chevers[1] undertook an "inquiry into the causes of death after injuries and surgical operations," and showed that on the condition of the internal viscera, and particularly of the liver, kidney, and spleen, depended in a great degree the probability of a successful or of an unsuccessful issue.

"I have long been perfectly convipced," he says, "not only that the greater proportion of deaths after wounds, in our metropolitan hospitals, result from the effects of disease in these organs called into activity by the accidents which the patients have undergone, but also, that any operation or wound, however trifling, will be extremely liable to prove fatal in persons whose kidneys are in any degree suffering from acute congestion, or from any condition at all approaching to that state;—and although my data are less complete upon this point, I believe that the same observation will hold good with regard to those who are the subjects of active splenic or hepatic disease."

Persons thus affected, Dr. Chevers reminds his readers, may be "in the prime of life, and to all appearances of robust and vigorous constitutions," and yet die after comparatively slight accidents or trivial operations, the real cause of the fatal issue only being made apparent by a *post-mortem* examination. A similar view of the subject has been recently advocated by Mr. Birkett, in an able and well reasoned paper, of which a short abstract was published in the number of this Journal for July, 1870 (p. 202), and both agree that "the chances of death after operations

[1] Guy's Hospital Reports, second series, vol. i., 1843.

appear to depend almost entirely upon the previous state of each patient's constitution."

Now, if it be granted (and we suppose no one will deny) that those who, in large towns, depend upon hospitals for surgical treatment—the sick poor—are less healthy and more apt to suffer from visceral disease (as the result of syphilis, intemperate habits, and many other causes) than the sick poor in small towns, in villages, and throughout the country; and if it be granted that large hospitals are, as a rule, found in large towns, and smaller hospitals, or none, elsewhere, our readers will, we think, agree with us that the differences in the death rates of amputations in different classes of hospitals, and in hospital as compared with country practice, *may* be explained by differences in the constitutions of those operated upon, and that such an explanation is at least as reasonable as that which would make the differences depend solely and simply upon the nature of the buildings in which the amputations are performed.

The second branch of the proposition under consideration, viz., that the difference in mortality is not to be explained by any difference in the nature of the injuries or diseases for which the patients are admitted, is of less importance, but we believe is also incorrect. We know of no injuries incident to civil life, which offer so unfavourable a prognosis as the severe compound fractures produced by the crushing of limbs under railway cars or heavily loaded wagons, and which are certainly proportionately commoner in large cities than in thinly populated districts. We believe also that amputations for cancer (the least successful of pathological amputations) will be found to be proportionately more frequent in large (metropolitan) hospitals than in either small (rural) hospitals or private country practice.

To estimate the mortality after amputation in country or provincial hospitals, Sir J. Simpson and Mr. Callender have each collected statistics, the former tabulating 3077 cases with 683 deaths (1 in 4.8), and the latter 2512 cases with 532 deaths (1 in 4.7); we confess that neither set of statistics seems to us to establish anything except (which is indeed pointed out by Mr. Callender) that the mortality is highest in those hospitals which derive their patients from the inhabitants of large though provincial towns, and least in those hospitals—no matter what their size—which are filled from the comparatively healthy populations of *bonâ-fide* rural districts.

Sir J. Simpson's third proposition is that "Limb amputations are more than three times as fatal in our large and metropolitan hospitals than [*sic*] the same operations are in private country and provincial practice." This startling proposition is founded upon the statement that 2098 country cases of amputation gave 226 deaths, or 1 in 9, while 2089 hospital cases gave 855 deaths, or 1 in 2½. We have already alluded to the doubtful accuracy of Sir J. Simpson's country statistics, and have given reasons for believing that 1 in 6 would be nearer the true death-rate than 1 in 9. On the other hand, 1 in 2½ we believe to be somewhat too large a mortality, even for British hospitals: we have published elsewhere a table of amputations derived from various sources, including 1492 cases from St. George's, St. Bartholomew's, Guy's, and University College Hospital. Adding to these, from Sir J. Simpson's pages, a more recent report from St. Bartholomew's (262 cases, 96 deaths), one from the London Hospital (224 cases, 106 deaths), and five from smaller hospitals also situated in London (153 cases, 70 deaths), we have a total from English metropolitan

hospitals, large and small, of 2131 cases, with 678 deaths, or 1 in 3.1, or 31.8 per cent. If we add further the statistics from the Edinburgh and Glasgow Royal Infirmaries (in which the mortality would appear to be exceptionally heavy) we have 3163 cases with 1098 deaths, or 1 in 2.8, or 34.7 per cent. In French hospitals the mortality, as shown by Malgaigne's and Trélat's statistics, is considerably higher—nearly 1 in 2, or 49 per cent.; but, on the other hand, in the hospitals of Boston, New York, and Philadelphia, is much lower, being for amputations in continuity, according to Dr. Chadwick, the latest writer on the subject, but a little more than 1 in 4, or 27 per cent.

If these figures are correct, it will follow that limb amputations are not more than three times as fatal, but only about twice as fatal, in large and metropolitan hospitals as in private country practice: the difference is, no doubt, sufficiently startling, but not, we believe, more than can be accounted for by the different circumstances under which the operations are performed in the two cases, and certainly does not justify the conclusion that hospitals are in themselves necessarily prejudicial to the interests of humanity.

Sir J. Simpson's fourth proposition is that "In country practice increased experience in amputations gives a still higher ratio of success to the results of the operations." This may well be (on the "practice makes perfect" principle), but we cannot see that it has any particular bearing upon the questions at issue: it is, it seems to us, if an argument either way, one in favour of hospitals, as the operators in those institutions have certainly, as a rule, more experience in amputating than is possessed by private practitioners.

The fifth proposition we are also willing to affirm, viz., that there is greater success in amputating observed in country practice, not only in limb amputations taken all together, but in the separate amputations, as of the thigh, the leg, etc., taken separately.

Proposition VI. is that "the contrast between the amputations in country practice, as compared with hospital practice, is proportionally more marked and pronounced in the slighter than in the greater amputations of the limbs," and this leads us to notice a fallacy in Sir James Simpson's argument, which did not escape the quick eye of Mr. Callender, viz., that Sir James's country statistics contain a much larger proportion of amputations of the upper extremity than do his statistics of hospital practice.

Propositions VII. and VIII. may be considered together: "Amputations of the forearm," we are told, "are about twenty or thirty times more successful in country practice than in the practice of our large and metropolitan hospitals," and "the deaths after amputation of the forearm in hospitals result, in the main, from those pathological causes which are usually ascribed to morbific hospital influences"—or, in other words, from "hospitalism." Now, that the true death-rate after amputation of the forearm in country practice generally is but 1 in 189 (as is here asserted), we cannot believe; nor do we believe that the death-rate of the same operation in city hospitals is quite so high as one in six—in St. Bartholomew's Hospital it is, according to Mr. Callender, but 1 in 21; in the Pennsylvania Hospital it is about 1 in 11. And, we must add, if the mortality in hospitals is due not to the previous constitutional condition of the patients, but to the morbific influences of the hospitals themselves, it is hard to understand why those morbific influences should manifest their effects proportionately more among those who are slightly, than among

those who are severely injured, and who would, therefore, one should suppose, fall more ready victims to "hospitalism's" attack. The true state of the case, however, is, we believe, simply this: that while amputation of the thigh or leg is, under any circumstances and in any locality, a severe operation, amputation of the forearm is, in a healthy countryman, a much more trivial affair than in a townsman of the class which fills our large hospitals, whether the amputation be done in a hospital or elsewhere.

Proposition IX., that double amputations are much more fatal in hospital, than in private country practice, does not, it seems to us, prove anything against hospitals; for the same causes which account for the difference of mortality after single amputations, will equally account for the difference in the case of double amputations, and a man who can ill support the loss of one leg, is of course still more likely to die when both are taken off.

Proposition X., that amputations upon septuagenarians are much less fatal in country than in hospital practice, may be acceded to; but the explanation, as justly suggested by Mr. Holmes, is, not that hospitals are to blame, but that, if the expression may be allowed, a countryman of 70 is, as regards vigor of constitution, commonly much younger than a townsman of the same age.

Proposition XI. has already been referred to in connection with Proposition II. Under this heading Sir. J. Simpson takes up categorically, and proceeds to answer, the various objections to his views upon Hospitalism, which have been advanced by Mr. Holmes. Most of these objections and answers have been already considered in the preceding pages, but there is one which seems to us to call for further comment: it is given here as Objection V. "'*I assert,' says Mr. Holmes, 'that such a comparison as Sir James Simpson has made between private and hospital practice must always be unfair, because the cases are selected on different principles in private practice' from what they are in hospital practice.* 'The fact' (he adds) '*must* be as I say,' for, to quote his own words, 'hospital surgeons refuse nothing in the way of operation which comes to their hands.' On the contrary," cries Sir James Simpson, "I feel and hope that, in the main, this argument is merely an illusion on Mr. Holmes's part. It would, indeed, be painful and revolting to most professional minds even to suppose that the poor, who form the patients in our rich hospitals, were cut and operated on by hospital surgeons on principles different from those on which patients were treated out of hospital." Dr. Brotherton, of Alloa, we are told, has also criticised Mr. Holmes's remarks upon this point.

Now, this is a plausible, sentimental kind of argument, which may commend itself to unprofessional minds, but which we confess we are surprised to find seriously advanced by such a man as Sir James Simpson. Every practical surgeon knows that, as a matter of fact, cases for amputation *are* differently selected in private and in hospital practice. In the latter, provided there is the slightest chance of saving life by an operation, the operation is recommended, and is usually readily acceded to by the patient or his friends; the reputation of the hospital, in an emergency, takes the place of a consultation of surgeons, and if the "head doctor" says an amputation is necessary, there is no one to gainsay it. In private practice there are many more formalities to be gone through with, and much clearer hopes of benefit from the proposed operation must, as a rule, be held out before it will be agreed to; this is particularly the case among the poorer classes,

with whom an unjust suspicion often arises that the doctor wants to make a profitable job out of his patient's misfortune—a suspicion which cannot, of course, arise in hospital practice, for here no question of a pecuniary nature enters at all into the calculation.

But this difference in the modes of selecting cases does not, as Sir J. Simpson and Dr. Brotherton seem to think, result in any disadvantage to the patients in hospitals. No surgical statistician will need to be told that a careful selection of cases for operation is not necessarily synonymous with the preservation of a large number of lives. This may be briefly illustrated as follows: suppose each of two surgeons, A. and B., is, in the course of professional experience, called upon to take charge of 100 patients with limbs hopelessly injured or diseased, so that in every case the only hope for life consists in amputation. Fifty cases in each surgeon's practice present such favourable conditions as to age, the state of general health, etc., that every reasonable prospect is afforded of obtaining a successful result; in these cases each surgeon amputates, and of the fifty patients saves forty. In ten cases in each category, the general condition of the patient is so low that there is no hope of saving life by any mode of treatment; here both surgeons decline to operate, and their patients, as expected, die. There remain then forty cases in each category which are on the border line; the patients are, humanly speaking, certain to die if let alone, and will very probably die even in spite of an operation; A., believing in the merits of a "good selection" of cases, holds his hand, and his patients die in due time as he had anticipated; but B., careless of his statistics, gives his patients the benefit of a doubt, amputates, and of the forty saves half. Which has done best? A., according to Sir James Simpson, for he has lost but one in five after amputation; but while the death-rate after his *amputations*, has been but *one in five*, the mortality among his *patients* has been *three in five*, while though B. (who "cuts and operates" on different principles from those acted on by A.) has lost *one in four* after his amputations, the mortality among his patients has been but *two in five*.

Sir James Simpson's twelfth proposition is as follows: "The country amputations, though far more successful, ought to be more perilous and fatal than the hospital amputations, inasmuch as they contain a marked excess of amputations for injury; which traumatic amputations are more dangerous than amputations for disease." Upon this proposition we have to remark that Sir James Simpson's statistics are radically defective in that they confound together *secondary* amputations and those for *disease:* the latter are indeed (as he tells us) less dangerous than *primary* traumatic amputations, but *secondary* traumatic amputations are, as we have elsewhere shown, much more dangerous than either, as well in civil as in military practice.

Proposition XIII. is that "The state of the patients at the time of operating in these (traumatic) amputations, is precisely the same in country practice and in hospital practice." This proposition is, we think, sufficiently answered by the considerations adduced in our remarks upon Propositions II. and XI. The patients, indeed, in both country and hospital practice, may be "in the condition of ordinary or normal health a few hours, or a very short time at least," before submitting to amputation, but the "ordinary or normal health" of a hardy peasant is, we insist, quite different from the "ordinary or normal health" of one whose home is a cellar or tenement-house in a large city.

Proposition XIV. is as follows: "Primary amputations are chiefly for the worst forms of compound fractures of the limbs; and the hospital surgeon of necessity sometimes operates in a less severe form of these injuries than the country surgeon, and hence ought to be more successful, but is not." This seems to us an unfair statement of the case; if the hospital surgeon sometimes amputates for less severe injuries than his country brother, it is because he has to deal with worse material, and because a given lesion is, as regards the chances of recovery, a positively worse injury in the one case than in the other. Moreover, we are disposed to believe, as has been already mentioned, that the worst cases of compound fracture are met with in greater proportionate frequency in hospital than in country practice.

As Proposition XV., Sir James Simpson declares that "In consequence of the relative severity of the cases of injury treated by amputation in the country, more die of simple shock in country practice than in large hospital practice." Of the deaths after amputation in the country cases collected by Sir James, thirty-six per cent. are said to have been from shock, while of 41 fatal cases which occurred in St. George's Hospital, but two, or five per cent., were attributed to the same cause. Hence, according to Sir J. Simpson, the country cases must have been the most severe. There are, it seems to us, several fallacies in this argument: (1.) Knowing, as we do, the very loose manner in which the word shock is used by many surgeons, and even by not a few surgical writers, we have no assurance that a large number of the deaths after amputation reported by Sir J. Simpson's country correspondents, were not in reality due to other causes—as for instance, to hemorrhage before, or perhaps even during, the operation; (2.) If the proportion of deaths from shock in country practice, and in that of St. George's Hospital, is accurately stated, it may be that the difference is due to the superior skill and judgment of St. George's surgeons in preventing death from shock, by adopting suitable treatment, and by choosing the proper stage of reaction for operative interference—the patients, though saved from immediate death, perishing at a later period from exhaustion, visceral disease, or pyæmia; (3.) Every practical surgeon knows that an extreme degree of shock sometimes accompanies a comparatively slight lesion, and hence that a large proportion of deaths from shock, in a given number of cases of injury, is by no means necessarily a proof that the injuries have been unusually severe.

Proposition XVI. is, that "Amputations of the thigh for disease are more common in infirmary than in country practice, but are three times more dangerous in hospital than in rural practice." Sir J. Simpson's statistics upon this point are, as we have already shown, defective, in that they confuse together amputations for disease and secondary amputations for injury. The mortality after thigh amputations for disease in hospitals, is, we believe, not nearly so high as is here represented, viz., thirty-seven per cent.; it is in British hospitals, according to the published reports of Guy's, St. Bartholomew's, and St. George's, but about twenty-five per cent., and in American hospitals, according to Dr. Chadwick's statistics, only twenty-two per cent.

To Proposition XVII., that "The causes of death after limb-amputations differ in some important points in large hospitals, and in private country practice," we are quite willing to agree; but we cannot see that this proposition proves anything one way or the other. According to Sir J. Simpson's statistics, in country amputations, the deaths from *shock*

are thirty-six per cent., and those from *pyæmia* less than five per cent. of the whole number, while in hospital amputations, the deaths from *shock* are but five per cent., and those from *pyæmia* between forty and fifty per cent.[1] of the whole number. But, as was long ago shown by Dr. Chevers, and as every practical surgeon knows, among the predisposing causes of pyæmia, none occupies a more important place than pre-existing visceral disease—disease not incompatible with an appearance of great health and vigour; and hence it need cause no surprise to find that deaths from pyæmia after amputation, are more common in city hospitals than in private country rooms, for, as we have already pointed out, latent visceral disease is more common among the poor of large towns than among dwellers in rural districts. Besides, as we had occasion to remark in considering Proposition XV., it is not impossible that a good many of the patients who in country practice died from shock within a few hours of the time of amputation, might, had they been received and promptly treated in a well-ordered hospital, have been tided over the first few critical days, and yet, dying at a later period, have come to swell the list of those who in hospital practice perish from pyæmia.

Proposition XVIII. is, that "The causes of death which Mr. Holmes points out as the reasons for the greater mortality of amputations in the Parisian as compared with the London hospitals, are the same causes that produce the greater mortality of amputations in our large British hospitals as compared with country practice." This proposition is, in one sense, no doubt true, but as applied by Sir James Simpson it seems to us to contain a palpable fallacy. The excessive mortality in Paris hospitals appears to be due to the great prevalence therein of erysipelas, pyæmia, etc., and to the same diseases is due, as we have seen, a large proportion of the deaths in hospital practice both in England, and we may add, in this country; but it is not proved, we insist, that the sole cause of the prevalence of these affections in either French, British, or American hospitals, is their defective hygienic arrangements; on the contrary, we believe that in each case the constitutional condition of the patients before entering hospital exercises a most important influence in predisposing them to the development of erysipelas and pyæmia, though it is highly probable that, as remarked by Mr. Holmes, the defective hygienic arrangements which are known to prevail in many of the French hospitals, may be likewise influential in this respect. In fact, as pointed out by Dr. Guy and Sir. J. R. Martin, there is reason to believe that the great mortality in Paris hospitals is due to a combination of these causes, the Parisian being, in the first place, "a worse subject for hospital treatment than the inhabitant of London," and, from the different systems on which hospital relief is conducted in the two cities, the Parisian hospitals, being "subject to be greatly overcrowded in unhealthy years and seasons," while the London hospitals "receive a number of patients, determined not by the wants of the population, but by their own resources."

Proposition XIX., that "Surgical patients in surgical wards seem sometimes to have pyæmia or surgical fever induced by the accidental inoculation of the morbific secretions formed in the bodies of other patients previously affected," is advanced as having been suggested some years ago by

[1] Sir J. Simpson, using a partial return from St. George's Hospital, gives 58.5 per cent., but Mr. Holmes's report of 300 cases gives only 50 per cent., while Mr. Bryant's (from Guy's Hospital) gives only 42 per cent. Dr. Chadwick's statistics (Mass. Gen. Hospital) give only 23 per cent.

Sir James Simpson, but is not supported by either evidence or argument; that pyæmia may occasionally originate in such inoculation we do not doubt, but such an occurrence is at least very rare, and in order for pyæmia to be developed under such circumstances, not only must there be an inoculation of "morbific secretions," but there must be, also, a state of system, in the recipient, predisposed to pyæmic infection.

Proposition XX. "The air breathed by patients aggregated in surgical wards becomes sometimes morbific and dangerous by its containing various inorganic and organic materials, and by the inmates mutually vitiating it more or less by the exhalations and excretions from their wounded, ulcerating, and sick bodies." That this is true of ill constructed and ill managed hospitals, we shall not of course deny, but it is surely not true of well constructed and well managed hospitals, and the faults of the bad are not to be adduced as an argument in favor of destroying the good. We are willing to go any lengths with Sir James Simpson in improving, whenever necessary, the hygenic conditions of hospital wards; but we cannot see that a hospital ward as such need necessarily be hygienically inferior to other apartments which sick and wounded men inhabit.

Sir James Simpson's twenty-first and last proposition is that "The residence in hospital wards of men in a state of relative bodily health is attended with a loss of life markedly greater than when the same class of men are not thus hospitalized, but allowed to reside in private cottages and dwellings." Granting this to be true—though we do not think it is proved by Sir J. Simpson's statistics of the death-rate among chronic lunatics in Scotland—we might advance as a similar and universally acknowledged proposition, the following: The residence in large cities of men in a state of relative bodily health is attended with a loss of life markedly greater than when men of the same class are not thus citizenized, but allowed to reside in country houses and villages. What then? Are we to give up living in cities? Are we to declare that because the death-rate is higher in cities than in the country, therefore cities, as at present constituted, are banes rather than blessings to society?

If the considerations which we have advanced are reasonable, and if our exceptions to Sir James Simpson's arguments have been well taken, our readers will, we think, agree with us that the distinguished Edinburgh professor's wholesale indictment of hospitals is not sustained by the evidence. And, after all, no practical end can, in our judgment, be attained by instituting a comparison between cases in private country practice and those in city hospitals. No one has, so far as we know, ever suggested that large city hospitals should be erected in thinly populated rural districts, and that men severely injured should be dragged scores of miles over rough country roads in order to submit to amputation within hospital walls; but for the surgical treatment of those for whose use hospitals are designed—the sick poor of large towns—hospitals are, we maintain (and in this opinion every one who has any experience of surgical practice in the courts and alleys of a city will coincide), not at all a bane, but in every sense of the word a blessing.

That hospitals, as at present constituted, are perfect, no one will pretend; on the contrary, all hospital surgeons are agreed that no effort should be spared, whether in the construction of new wards or in the repair and alteration of old ones, to attain and maintain the highest possible degree of sanitary excellence, by the adoption of all means which may be proved by the study of hygiene to be appropriate to this end.

To discuss fully the precise mode or modes in which the sanitary condition of our present hospitals may be most readily brought to, and kept at, the high standard indicated, would require more space than can be afforded by the limits of this review. Sir James Simpson's original suggestion that the hospitals themselves should be torn down and substituted by "wooden, or brick, or iron villages" may probably be set aside as simply impracticable; even were there space in large cities in which to erect such villages, their proper *administration* (in the technical sense of the word), would be an impossibility. Whether the sought-for panacea is to be found in knocking out the wall on one side of every staircase and corridor; in doing away with corridors altogether and building outside staircases to all the upper wards (both of these plans are suggested by Sir James); in douching all our wounded patients with the spray of carbolic acid and otherwise adopting the minutiæ of what is known as the "antiseptic method;" or in burying our wounded beneath the soil of mother earth, and erecting over them a cerulean canopy of blue paper; whether in any of these plans is to be found a sovereign remedy for the evils of hospitals, and a sure prophylactic against "hospitalism," may be doubted. The subject is too extensive to be entered upon at present, but may perhaps form matter for further discourse upon some future occasion.

J. A., JR.

ART. XVII.—*Lehrbuch der Pathologischen Gewebelehre zur Einführung in das Studium der Pathologischen Anatomie, verfasst* von Dr. EDUARD RINDFLEISCH, Professor der Pathologischen Anatomie in Bonn. Zweite Auflage. 8vo. pp. 633. Leipzig, 1871.

A Text-book of Pathological Histology. An Introduction to the Study of Pathological Anatomy. By Dr. EDWARD RINDFLEISCH, Professor of Pathological Anatomy in Bonn. Translated from the second German edition, with the permission of the author, by W. C. KLOMAN, M.D., assisted by F. C. MILES, M.D., Professor of Anatomy, University of Maryland. 8vo. pp. 695. Philadelphia: Lindsay & Blakiston, 1872.

PATHOLOGICAL ANATOMY is one of the youngest branches of medicine, and may be fairly regarded as having had its starting-point in the work of Morgagni (1761), "On the Seat and Causes of Disease as traced out by Dissection." In the century which has since passed, pathology has made great advances, and this is more markedly the case since microscopic investigation has furnished a more accurate basis for the classification of diseased structures, and has caused pathological histology to assume in relation to morbid anatomy a position much more important than that held by normal histology to anatomy. Its province, as defined by our author in his preface, is to show how such gross changes in organs as enlargements, diminutions, indurations, softenings, discolorations, etc., have their origin in certain changes of their structural elements, and to explain the former by the latter.

The great strides made by science in Germany during the last twenty years, is nowhere more manifest than in this department; and the patient and unwearied investigations, and the beautiful and elaborate monographs

published at various times by Virchow, Recklinghausen, Cohnheim, Stricker, Thiersch, Waldeyer, Köster, and others, may well put other countries to the blush when we compare with them the work done, during the same period, either in Europe or in this country. In fact the additions to our knowledge in this direction have of late been so rapid that in this, the second edition of his book, Prof. Rindfleisch has been obliged entirely to rewrite that portion of it devoted to morbid growths. The book differs agreeably from many text-books in being not simply the work of a compiler and bookworm; every chapter bears the stamp of the author's individuality and laborious investigations—here testing the observations of others, there adding some contribution of his own. In a word, as he informs us in his preface, and as the numerous original wood-cuts amply testify, the work has been written rather at the microscope than in the study.

In a few crisp words of "Introduction," Rindfleisch brings us, with almost painful distinctness, face to face with the limits of our histological knowledge, by showing how, when we observe the cells of any living tissue, even with the best optical instruments, we are unable, so long as they are in a state of health, to trace any changes whatever in their appearance; so long as the cell is in a state of formative equilibrium—its accumulation being exactly sufficient to balance its waste—so long it presents to us the same appearance, and we infer nutritive changes only because while certain materials are continually taken up into the system, others are constantly cast out, which prove to be "metamorphic products of vitalized substances."

Any changes which we perceive have two principal directions—formation and degeneration; and in most morbid processes we find these side by side.

It is impossible to criticize justly in the brief space at our disposal a book which is written in so fair and impartial a spirit, and which is, in itself, an epitome of the present state of pathology. The reviewer will, therefore, confine himself to a mention of some of the more noteworthy portions of it, in the hope that every one really interested in pathology will provide himself with a copy, and read it at his leisure.

The book is divided into two parts, general and special.

The first treats of the Decomposition and Degeneration of Tissues and of Pathological New Formation.

The second of Anomalies: I. Of the blood and the places of its formation. II. Of the circulating apparatus. III. Of the serous membranes. IV. Of the skin. V. Of the mucous membranes. VI. Of the lungs. VII. Of the liver. VIII. Of the kidneys. IX. Of the ovaries. X. Of the testicles. XI. Of the breasts. XII. Of the prostate. XIII. Of the salivary glands. XIV. Of the thyroid. XV. Of the suprarenal capsules. XVI. Of the bones. XVII. Of the nervous system. XVIII. Of the muscles.

The first part opens with an account of the decomposition and degeneration of tissues, and takes up the study of necrosis. It shows how, after either general or local death, our tissues fall a prey to the large quantity of water (81 per cent.) contained in them; the albuminates and their derivates are washed out of their containing cells. The blood rapidly changes; its colouring matters leave the corpuscles, and are imbibed by all surrounding tissues, causing the well-known cadaveric discolorations (staining); the red discs become colourless, change their form,

and finally disappear. All nucleated cells change rapidly, the protoplasm coagulates and becomes cloudy, and both nucleus and cell break up into relatively large granules, which finally become cloudy and disappear. We are most familiar with this phenomenon in the death of the striated muscles, the so-called rigor mortis. Here, as Kühne and others have shown, the stiffness depends on the separation of a solid albuminous substance (myosin) from the muscular juices, thus forming a whitish but little transparent mass, which causes a marked opalescence, contrasting with the brownish discoloration of other portions. This, however, is but the first step towards death of the muscle, as is instanced by its artificial production in a frog's thigh, when we ligature the supplying artery, but which disappears if the blood supply be restored. If, however, it persist long, the muscle loses its colour and consistence, the transverse striæ and nuclei disappear, numerous dark points, fat globules and pigment granules are seen, the disks disintegrate, and finally, also, the sarcolemma giving way sets free a granular detritus. In some forms of gangrene, however, the muscle breaks up into the Bowman's discs. The non-striated muscular fibres present somewhat similar appearances, as is seen in the so-called softening of the stomach, which was formerly considered as a sign of disease, but is now known to be only a post-mortem change. Bone, as is well known, remains in the necrotic process relatively unchanged; but the fact that sequestra are of less specific gravity than the normal dried bone, shows that it has not only lost its marrow and bloodvessels, but that also a considerable portion of the organic matrix of the bone cartilage is lost by putrefaction. The chemical changes which ensue are manifold, but present us with a number of interesting forms; thus gangrene of the lungs, liver, spleen, etc., often presents us the opaline concentric rounded forms of leucine. Tyrosin shows itself in fine, satiny white needles; margarin in the well-known crystallizations in the fat vesicles, and also in the free oil globules. Occasionally, in alkaline or neutral putrid fluids, we see the coffin-lid-like crystals of the ammonio-magnesian phosphate, further pigment granules and vibriones. The latter our author ranks among the vegetable parasites, and asserts, with Frau Johanna Lüders, that these minute masses of protoplasm escape from the spores of various fungi.

The next section treats of those forms of retrograde metamorphoses which our author calls conditions of involution, viz.: Fatty degeneration, cloudy swelling, mucoid softening, and colloid degeneration. In the first named process we have numerous minute fatty molecules making their appearance in all parts of the cell, within the nucleus and protoplasm; as these become more numerous the cell becomes more spherical in shape, and assumes the appearance of a granular corpuscle. We have the prototype of this process in the formation of milk; here the yellowish granular corpuscles are called colostrum corpuscles, and have their origin in the fatty degeneration of the epithelial gland cells; and in ordinary milk the white appearance is due to the further breaking up of these corpuscles into their component globules. We see the same process in the degeneration of the epithelial cells of soft cancers, where we so frequently, by scraping the cut surface of the tumour with a scalpel, obtain an abundance of a cream-like cancer-juice.

The so-called cheesy degeneration is another form of fatty degeneration, and occurs in those tissues most readily where, either from their position in the body, or from their naturally small supply of blood, there

is not present fluid enough to allow of the breaking up and floating off of the fatty molecules and cell detritus. The cells desiccate and shrivel into small angular shapes, and finally we have a yellowish-white friable smeary mass, which was formerly believed to form an essential part of tuberculosis, and was called "crude tubercle."

We must steadily keep in mind that fatty degeneration is something entirely distinct from fatty infiltration; here the fat does not come from without, but is a visible liberation of the fat formerly incorporated in the cell protoplasm. A muscle, therefore, in a state of fatty degeneration, yields no higher percentage of fat than a healthy one. It is simply a rearrangement of the constituents of the cell, and as in all young cells where active growth is going on (*e. g.* in the cells of the yolk in the fecundated ovum) a considerable number of fat molecules are seen in the albuminous protoplasm of the cells—fat which entirely disappears in the formed tissue; so here, in its retrograde metamorphosis, we see it once more given up and becoming visible.

In both mucoid and colloid degenerations we have a diaphanous, trembling, jelly-like substance, penetrated by net-like fibres, which consist in the first case of stellated anastomosing cells; in the second, of the remains of the fibrillar intercellular substance. The first contains mucin, and is, in all respects, constituted like the fœtal subcutaneous connective tissue, and the Whartonian jelly of the umbilical cord. The second contains a substance not precipitated, like mucin, by acetic acid, and the microscope shows the presence of a highly refracting, fat-like body, the so-called colloid globule in the cells of the part.

The interferences with nutrition caused respectively by the amyloid, calcareous, pigmentary and fatty infiltrations are next considered. When we examine a portion of amyloid infiltrated tissue, we find that the cells look swollen, that their sharp angles and edges have been rounded, and that they have a homogeneous, translucent, and slightly opaline appearance. In the spleen, kidney, liver, and other organs usually affected by this change, we invariably find that its first traces are to be seen in the smaller arteries and capillary vessels. In the arteries the infiltration first attacks the inner, then the middle coat, and finally the adventitia and the adjacent tissue. This diminution of the calibre of the vessels causes the anæmic state of such tissues, while the infiltration of the cells produces the well-known grayish translucent appearance. The infiltrated tissue loses its elasticity, becomes doughy, and pits on pressure. As is well known, the addition of iodine causes the infiltrated parts to turn mahogany-red, and thus furnishes a valuable test for the slighter degrees of the affection. In treating of calcification, our author defends the view that the solubility of the lime-salts in the blood and tissues is due to an excess of carbonic acid, and careful observation shows that the first deposits of these salts, both in physiological and pathological processes, occur at the extreme boundaries of some given vascular zone. The lime is there deposited because its solvent, the free carbonic acid, has, by its greater capacity for diffusion, left the stagnating blood and been excreted in other ways, leaving the insoluble lime-salts behind.

True, pigmentation (not owing to some dyscrasia) is usually associated with some local hyperæmia or extravasation of blood, and is due to the escaped and degenerated hæmoglobin. This changes into hæmatoidin, which is often met with in its crystalline form, but afterwards breaks down into dark brown, or black granules, which are taken up by the pro-

toplasm of cells, leaving their nuclei free, and thus reversing the action of many of our staining solutions, *e. g.* carmine. The pigment infiltration does not seem materially to diminish the functional capacity of the cell. Thus we all know the facility that the cells of the choroid and those of the negro's skin have for keeping themselves supplied from the colouring matter of the blood, and the pigmented sarcomas exhibit no diminution of their malignancy, even when thoroughly infiltrated with black or brown pigment.

The fatty infiltration is so familiar to all, as it occurs in the liver and subcutaneous cellular tissue, that a detailed description of it is superfluous. It distinguishes itself from fatty degeneration in the fact that although both commence as small globules in the interior of the cell protoplasm, in fatty infiltration these coalesce into large drops, and force the nucleus to one side, while in fatty metamorphosis, although they may steadily increase, they never become confluent.

Pathological New Formation.—After a few general remarks on cell structure, origin, and growth, and a series of definitions of the terms he subsequently uses in the work, our author gives an interesting section on "normal growth as a type of pathological," and commences the consideration of pathological new growths by taking up the subject of inflammation. Realizing that the whole theory of inflammation has recently undergone a revolution, and that it is really one of the most knotty and difficult, as well as one of the most important subjects in pathology, our author is very circumspect and guarded in the opinions which he enunciates regarding it. While accepting, without reservation, the discoveries of Cohnheim, so far as relates to the migration of the white corpuscles out of the vessels, he hesitates to accept them as the only source of new growth, quoting the investigations of Stricker to prove that the cells of inflamed tissue themselves undergo changes in inflammation "which can only be interpreted in the sense of progressive metamorphosis," and also, the further observation of Stricker, that the wandering cells in inflammatory depots multiply by fission. Moreover, in his chapter on the inflammation of mucous membranes, he refers to the observations of Remak and Buhl, confirmed by his own experience, which prove that in such inflammations there is frequently an endogenous formation of cells by the cells of their lining epithelium. In treating of the healing of wounds by granulation, he shows that the wound does not skin over by outgrowth of the neighbouring epithelium, but that, influenced by a certain epithelial infection, the cells nearest the remaining epithelium gradually take on an epithelial character, and adds: "If we seek for an analogue of this epithelial formation, we can only compare it to that primal separation of the embryonal germinal tissue into epithelial and non-epithelial layers, and regard it as a genuine embryonal differentiation."

Under the head of Specific Inflammation, Rindfleisch treats of those new growths which are classified by Virchow in his work on "Morbid Tumours" as granulation and lymphatic tumours.

These are to be distinguished from ordinary inflammation in that, in place of or in addition to the ordinary plastic exudation, they yield certain products, which are characterized by special anatomical peculiarities. The latter are typical for each individual process, and are in general traceable to the particular quality of the inflammatory irritant. As such, in every case, a specific virus is active, which either passes into the organism by hereditary transmission, or is acquired by contagion, or finally

primarily formed in the organism. Under this head syphilis, lepra, glanders, typhus, and tubercle, are specially treated of.

Constitutional syphilis gives a tendency to active hyperæmia and inflammatory new formations. The so-called gumma syphiliticum is due to the formation of a circumscribed globular swelling within a larger depot of germinal tissue. The so-formed node differentiates itself by marked metamorphosis, and while the bulk of the germinal tissue goes into the formation of a dense cicatrix with unusual retractile power, the cells within the gumma retain a rounded form, and by their anastomoses form a cellular net, the basis substance undergoing a mucoid degeneration. The cells themselves undergo fatty degeneration, and thus become transformed to rounded aggregations of fat granules. Thus is formed the characteristic gummy tumour—an elastic yellowish-white rounded mass, imbedded in a depot of newly-formed connective tissue.

Typhus.—We have the acme of typhus changes in the well-known changes of the Peyer's patches in enteric fever. The cells differ from those of ordinary acute inflammation in that they have a more epithelial appearance, and while, in the ordinary lymph corpuscle, the protoplasm scarcely equals the space occupied by the nucleus, in the "typhus cell" the protoplasm occupies usually more space than the nucleus, thus approaching a low grade of epithelial development.

Tubercle.—The modern definition of tubercle excludes all that cheesy degeneration which was formerly tubercle *par excellence*. We now restrict the term to the grayish, translucent, compact and miliary nodules which are so frequently associated with the cheesy inflammation. According to Villemin, Klebs and Cohnheim, the introduction of this cheesy detritus into an individual results in the occurrence of "miliary tuberculosis," whether this result be obtained by inoculation or the formation of the cheesy detritus within the individual himself. If we tease to pieces, with fine needles, one of the miliary granules, we obtain large cells of a rounded angular form, consisting of a finely granulated, highly refracting substance. These have a moderate sized, usually eccentrically situated nucleus, shining and darkly contoured—sometimes single and sometimes multiple. We encounter also smaller cells, in every way similar to the nuclei of the larger ones, which have probably been formed by endogenous development. These are all imbedded in a finely fibrous network. Experience proves that such tubercles arise usually in the outer coat of the small arteries and transition vessels. "They are placed as lateral projections, single or united in small groups upon the larger branches, while upon the more minute branches and the 'almost capillary vessels' they present themselves as spindle-shaped varicosities, which occupy the whole circumference of the vessel." This is often admirably seen in the small cerebral vessels where His has demonstrated that the tubercles arise from the endothelia of the lymph vessels which here surround and form a sheath for the bloodvessels. The presence of a bloodvessel is, however, not a necessary condition for their origin; they may arise anywhere where we find an endothelium—from the endothelia of serous membranes, of lymph vessels, of bloodvessels—in short, wherever endothelial cells are subject to a specific irritation.

Histoid Tumours, according to our author, include the sarcomata, and the cavernous, fatty, cartilaginous, mucoid, bony, muscular, and nerve tumours, and are distinguished by the interior continuity of their elementary parts. "The majority of them consist only of one tissue; when,

however, more tissues participate in their composition, they are never found in a differential apposition with sharply drawn limits, as epithelium and connective tissue, but pass organically into each other." They all have for their starting-point embryonal connective tissue. This group includes tumours which are clinically of the most dissimilar nature—some types of benign growths, *e. g.* lipomata, others of malignant ones, *e. g.* the small round-celled sarcomata. Where such new formations grow only by a continuous proliferation of their own cells, they often after they have attained a certain degree of development are enveloped by a fibrous capsule which completely separates them from the surrounding tissues, and we constantly meet fatty, cartilaginous, and fibrous tumours thus isolated. Where the growth is a peripheral one, the tumour increasing constantly at the expense of the surrounding connective tissue, as in the sarcomata, we have to deal with a much more aggressive and dangerous form of nutritive disturbance.

The sarcomata present types of tissue such as are constantly met with in inflammatory hyperplasia, viz., the round-celled formative tissue of granulations, the spindle-cell tissue, and the compact fibrous tissue of cicatrices, and we accordingly distinguish in them three principal branches, namely, the round-celled sarcoma, the spindle-celled sarcoma, and the fibroma. Some varieties of sarcoma are among the most malignant tumors with which we are acquainted, and Rindfleisch here enters into a discussion as to the malignity of new formations and sums up the arguments of those who believe that the development of every malignant tumour is preceded by a constitutional dyscrasia as well as of those who advocate a primary local origin, and is evidently inclined to range himself with the latter party, although he is not disposed "to anticipate in any way the coming decision of this question." As a general rule it may be stated that those growths are the most malignant which take their origin in tissue where there is a large vascular supply, and that those varieties which contain many small, young, mobile cells, recur more frequently and more promptly than those containing spindle-shaped or myeloid cells.

Carcinomata.—The term cancer, which was formerly applied to all malignant growths, is now restricted to those which are essentially epithelial in their type. They consist of an alveolar fibrous stroma in which are packed cells of an epithelial type without any intra-cellular substance. "By far the greater number of carcinomas proceed primarily from the epithelial clad surfaces of the body, from the skin and mucous membrane, or from the secreting glands. They depend upon an abnormal growth of epithelial tissue. We have, therefore, a variety of processes from simple hypertrophy of a gland to well-marked cancer, and modern pathologists have invented the term adenoma to designate a form of tumour which stands between the two.

The greatest diversity of opinion reigns as to the manner in which normal epithelium is reproduced and grows, and the question is even more hotly contested as regards the pathological formations of an epithelial type. Thirsch, Waldeyer, Köster, and others have recently published able monographs on the subject, but much remains yet to be accomplished.

Rindfleisch divides the carcinomas into I. Glandular cancer; II. Epithelial cancer (epithelioma). The first may be either hard (scirrhous) or soft (medullary), according to the greater or less abundance of the connective tissue stroma, and according to greater or less size of the contained

alveoli, and in certain situations (*e. g.* stomach, large intestine, liver, etc.) we sometimes find the cells undergoing the colloid degeneration. In the glandular variety we are told "that the new formation has its starting-point in the glandular epithelium has of late been established by numerous investigations." The cells multiply by division. First of all, probably, the lumen of the tubuli and acini fills up and in their place we have solid cell groups, which, however, immediately push out outgrowths in all directions, and by means of these penetrate into the surrounding connective tissue. Under the head of epithelial cancer our author recapitulates the results of some of the more recent investigations on the subject and says, "According to Thirsch, Billroth, and others, the epithelial shoots grow exclusively by the division of the epithelial cells contained in them; according to Köster, the growth is by apposition and ensues by a corresponding metamorphosis of the endothelia of the lymph-vessels in whose cavities the epithelial projections exclusively extend themselves and advance. I for my part can neither overlook the very evident fact that lively nuclear and cellular divisions take place in the epithelial shoots, nor ignore the speaking pictures which we obtain if we, according to Köster's direction, cut parallel to the surface and examine the indurated edge of certain flat epithelial cancers of the cutis."

In the second or special part of the work, Rindfleisch takes up in succession the various subjects whose headings have been enumerated in the first pages of this notice. In the chapter on the blood, we have an able description of the minute structure of thrombi; of the methods in which they originate in the vessels and subsequently soften or become organized, and also of the embolic processes.

In the chapter on serous membranes our author states, that these structures are not organs, but part of that system of clefts or fissures in the body, which, since the investigations of Recklinghausen, have been universally regarded as forming a part of the lymphatic system. The epithelium covering their surface consists of thin transparent polygonal scales, and attached to it on the under side a nucleus with barely enough protoplasm to hold it in position. He maintains that in inflammation these cells proliferate and subdivide, and that from them the primary heteroplastic growths (sarcoma, tubercle, cancer) in these membranes originate.

At the commencement of the chapter on the anomalies of the mucous membranes, our author recapitulates in a few words their peculiarities of structure and function and calling attention to the thinning of the epithelial layer by the loss of its horny stratum as we pass at any of the orifices of the body from skin to mucous membrane, thus continues, "No doubt can reasonably exist as to the significance of this decrease of the thickness of the epithelium in the anterooms (atria) of the digestive tract. It is the first step towards facilitating the osmotic interchange between the fluids and gases in the lumen of the tract upon the one side and the blood upon the other. Where this exchange, as resorption or secretion, becomes more active, where it forms the foundation of the entire nutrition, the last layer of pavement epithelium also disappears and the cylindrical cells alone remain. Thus the intestinal canal from the cardia to the anus, thus the respiratory passages, thus the female sexual organs from the external orifice of the uterus, are all clothed with a cylindrical epithelium." "The greater part of all the diseased conditions of man are either catarrhs of mucous membranes or are complicated with them." Then fol-

lows a full discussion of catarrh, and he divides all other inflammations of mucous membranes into croupous and diphtheritic. In the former we have a fibrinous exudation on the surface, which, when removed, shows the mucous membrane remaining intact; in the latter the exudation takes place in the substance of the tissue itself, and the apparent false membrane is really a slough of the whole thickness of the membrane, leaving a raw surface beneath. In pneumonia we have a type of the first; in dysentery of the second. It will be seen at a glance, that according to this definition, diphtheritis is something very different from what is usually understood in clinical treatises as diphtheritic inflammation in the throat and chest.

We hope that the foregoing *résumé* and extracts will have been sufficient to give the reader a fair idea of the book, and that it may have interested him sufficiently to induce him to carefully study it. In conclusion, we cordially recommend it as the best treatise on the subject; a treatise which, if studied in conjunction with either Förster's *Handbook of Pathological Anatomy* or Klebs's work of the same title, cannot fail to give the reader a clear and comprehensive insight into pathology.

As regards the American translation we have only to add, that while it does credit to the gentlemen whose names stand upon its title-page as a most conscientious rendering of the original, it is disfigured by arrant "Germanisms" at almost every page, and we hope that should another edition be called for they will exert themselves to render it into more vigorous and idiomatic English. W. F. N.

ANALYTICAL AND BIBLIOGRAPHICAL NOTICES.

Art. XVIII.—*Guy's Hospital Reports.* Edited by C. Hilton Fagge, M.D., and Arthur E. Durham. Third series. Vol. XVII. 8vo. pp. xviii., 536. London: J. & A. Churchill, 1872.

The present volume of this unequalled series of Hospital Reports contains fifteen papers, which, in accordance with the custom of previous years, we shall notice in succession, so that those of our readers who have not access to the book itself may yet become acquainted with its contents, which are of considerable variety and of great practical value. Grouping together, first, those papers which are particularly interesting to surgeons, we shall begin by inviting attention to Art. I., which consists of a third instalment of Mr. Poland's elaborate *Statistical Report on the Treatment of Subclavian Aneurism.*

The modes of treating subclavian aneurism which are considered in this part of Mr. Poland's essay are, (1) Ligation of the first portion of the subclavian artery; (2) Simultaneous ligation of the first portion of the subclavian and of the common carotid artery, and of both these vessels with the vertebral artery; (3) ligation of the innominate artery; and (4) simultaneous ligation of the innominate and carotid—this mode of treatment being illustrated by but a single case (that of Dr. Smyth, of New Orleans), in which the vertebral artery was subsequently tied for secondary hemorrhage. The details of this now famous case, which Mr. Poland calls "the only one on record of recovery after an operation for the cure of subclavian aneurism," are familiar to our readers from having been published in the *Domestic Summary* of the number of this Journal for July, 1866, pp. 280 *et seq.* Ligation of the first portion of the subclavian artery, whether by itself, or in connection with the simultaneous ligation of other vessels, has invariably proved fatal when performed as a Hunterian operation, as of course it must be in the case of subclavian aneurism. We are interested, however, to observe that, as a *distal* operation, it would appear that ligation of the first part of the subclavian might be successful: this is shown by a case here quoted from the record of Hobart, an Irish surgeon, in which the first portion of the subclavian and the carotid were simultaneously tied for an aneurism supposed to be of the innominate (though afterwards found to be aortic), and in which the subclavian ligature came away safely on the fourteenth day, though fatal hemorrhage from the carotid occurred at a subsequent period. After death "it was found that *perfect union had taken place where the ligature had been applied on the subclavian artery.*" We must add that we can scarcely conceive of a case calling for the distal operation, in which the subclavian artery should not be tied in its third rather than in its first portion.

To all who are interested in the statistics of the surgical treatment of aneurisms—and what surgeon is not?—we would commend a careful study of Mr. Poland's pages; we look eagerly for the next and concluding portion of his "Report," which we do not hesitate to declare will form, when completed, the most valuable contribution ever made to the literature of its subject.

The next paper to which we shall invite our readers' attention is Art. VI.

On Skin Grafting, by THOMAS BRYANT. Mr. Bryant has had considerable experience with the mode of treating ulcers introduced by Reverdin, and his views coincide with those of Mr. Pollock as to the best method of performing the operation. (See number of this Journal for January, 1871, p. 277.) In referring to Reverdin's query, "Is the growth of skin due to the effect of contact or neighbourhood, or is it due to the proliferation of the transplanted element?" Mr. Bryant says:—

"How the engrafted pieces act in the healing process is not yet satisfactorily settled, and Reverdin's questions, as already quoted, have not yet been completely answered. That they act as direct stimulants to the sore itself, and more particularly to the margin of the sore, there can be but little doubt, for as soon as the 'grafts' have taken, the margin of the sore nearest to them is seen to cicatrize and to send out prolongations of new cicatrizing tissue to meet similar prolongations from the new cutifying centres; the sore in this way becoming subdivided into smaller sores, and then rapidly healing. That the engrafted portions grow by the proliferation of their own cells is likewise proved by the fact that in the case of a white man, upon whose ulcerated leg I engrafted four small pieces of black skin, the whole being no larger than a barley-corn, the black skin grew twenty-fold in ten weeks, . . . the black portions gradually enlarging, and sending out prolongations which joined till one patch of black skin had formed, . . . the ulcer healing as rapidly where the black skin was grafted, as where the white was placed."

In one case Mr. Bryant transplanted a piece of skin upon the everted mucous membrane of an exstroverted bladder; the graft "almost doubled its natural size, but no more; it did not exert any skin-forming power on the mucous membrane on which it was placed." Two lithographic plates (one coloured) and two woodcuts accompany this short but interesting communication.

We come next to the longest paper in the volume, Art. VIII., *Clinical Records*, by J. COOPER FORSTER. A record of one hundred and forty-three very miscellaneous cases, spread over no less than one hundred and fifty-six octavo pages, and without either table of contents or index, is almost as little likely to be consulted, and is almost as effectually buried, as if it had been permitted to slumber in Mr. Forster's manuscript note-books; we have always entertained a great respect for case-taking, and confess to a fondness for case reading, but we are staggered by Mr. Forster's pages. Of the one hundred and forty-three cases, some, to say the least, are less interesting, and less urgently demand publication, than others, and we cannot but think that surgical science would not have been the loser, had Mr. Forster adopted a more rigid rule of selection. In noticing the last volume of the Guy's Reports, we ventured to express a fear that Mr. Forster's zeal for clinical records might diminish, and that as the newness of his broom departed, its work might be less thoroughly accomplished; but we are already forced to acknowledge our error, for he has this year not only swept his ward clean, but has smothered us in the dust.

Art. XII., *On Embalming*, by H. G. HOWSE, M.S., is of considerable practical interest. From Mr. Howse's description of his mode of preserving anatomical subjects we extract the following:—

"I consider glycerine to be *the* preservative fluid. It does not, however, prevent maggots from breeding in the muscles, nor does it prevent fungus from forming on the dissections when left exposed to the air. To prevent the former, I used to employ, in all the earlier experiments, a watery solution of arsenite of potash, freely injected, before the glycerine. I soon found a practical inconvenience from this, in that it made the subject slightly œdematous, thus requiring more glycerine afterwards to saturate it, and making the injections more expensive. For this reason I tried to prepare a glycerine solution of arsenic. Arsenious acid does not dissolve in this medium in the cold, or only to

a very slight extent. When the glycerine is heated to destructive decomposition, *i. e.* to ebullition, other chemical compounds get formed, and then the arsenic dissolves freely. . . . It is not wise, however, to make it take up much more than a pound to the quart. . . . It should be filtered before using. . . . The injection is always made from the femoral artery, opened at the apex of Scarpa's triangle. Two injection pipes, provided with stop-cocks and with long nozzles, are inserted, one passing up and the other down the artery, and a piece of strong ligature-silk tied tightly around it, to prevent the glycerine escaping. . . . The siphon arrangement is always made use of in injecting, *i. e.*, a long piece of india-rubber tubing is tied over the injecting pipe, and communicates with a reservoir placed considerably above the level of the body. To make the siphon arrangement act, this tube must of course be filled with fluid before inserting the end into the reservoir, and as the viscid glycerine takes a long time in doing this, I usually fill it first with a watery solution of the usual preservative (arsenite of potash), keeping the stop-cock on the injecting tube closed, then inserting the mouth of the tube into the glycerine, and opening the stop-cock. The glycerine injection will now slowly flow. If the arsenical glycerine fluid be made as above recommended, it will be sufficient to inject a quart and a half of it first, corresponding with about one pound and a half of arsenious acid (the injection being completed with pure glycerine). This will be sufficient to prevent maggots breeding in the subject even in summer time. . . . As regards the quantity of glycerine to be used, it will vary with the size of the subject, *i. e.* with the *bulk* of it. . . . The average quantity that we have expended upon our subjects is about three gallons each. Great care must be taken in keeping the cistern and the pipe full of fluid. . . . If, unfortunately, during the progress of the injection, the pipe should become empty, it is best filled up again with arsenical watery fluid. . . . I have said that it is necessary to *saturate* the subject. This is best done in the following way: After about two gallons have been injected it becomes œdematous; it should then be allowed to stand for two or three days, during which blebs form on the surface; these being pricked, a large quantity of fluid drains away. The other gallon of glycerine being then injected, acts much more effectually in the preservation of the subject than if it were all injected together. . . .

"Although the arsenical glycerine quite prevents maggots breeding in the subject, it does not prevent the formation of mildew in very hot weather. . . . For preservation, then, during the hot months, I believe it is best to keep the subject wrapped in cloths soaked in carbolized glycerine, and standing in a tray, covered about half an inch deep with similar fluid so that the cloths may remain constantly moist by capillary attraction. After the glycerine injection is completed, the arteries may be filled with the usual paint or coloured paraffin injection."

The last paper particularly addressed to surgeons is Art. XIII., *A Description of the Appearances of the Human Eye in Health and Disease, as seen by the Ophthalmoscope. Sixth Series, Congenital Anomalies.* By C. Bader.

The anomaly described in this paper is the existence of "a brilliant opaque-white, more or less well-defined substance upon or adjoining the optic nerve, hiding from view the parts beneath, and, more or less, the blood-vessels which pass through the white substance. The course of the retinal bloodvessels and the refraction, as ascertained by direct examination with ophthalmoscope, show that the white substance projects beyond the surface of the adjoining retina. The white colour . . . is thought to be caused by white opaque tissue connecting the retinal elements and optic nerve fibres (similar to the connective tissue in the optic nerve outside the eye). Whether the elements of the retina are healthy among this opaque connective tissue is undecided. . . . It does not seem to give rise to any appreciable impairment of sight, which explains its being met with only accidentally; it has been observed at all ages, in eyes of all shapes (more often, perhaps, in hypermetropics); it often occurs in both eyes of the same person."

Mr. Bader's paper is adorned with a chromo-lithographic plate containing five figures. J. A., Jr.

We shall now invite the attention of our readers to the medical papers of the volume, and first to

Art. II. *Cases of Disease of the Nervous System, with Remarks.* By SAMUEL WILKS, M.D., F.R.S.

A Case of Aphasia, with Remarks on the Faculty of Language and the Duality of the Brain.—The woman, the report of whose case is accompanied by some very interesting remarks, was 24 years of age and a domestic servant. Without ever having had any previous illness, she was found, a fortnight before she came under the author's notice, speechless and paralyzed on the right side. She improved rapidly under treatment, so far as the paralysis of the limbs was concerned, but although it was evident that she understood written and spoken language perfectly, and although she read books and the newspapers and seemed as rational as any other person in the ward, speech did not return. It was then attempted to teach her to speak afresh. A box of letters of the alphabet, with pictures upon them, was brought to her, and she was taught in the same manner as a child, or as a person learning a foreign language. In repeating to her several times the name of one of the objects on the cards, she would at length articulate the name herself. She sometimes forgot the name of an object she had once learned, and would miscall it, but she never under these circumstances used a word which she had not learned since her illness. When given a number of letters and asked to arrange them into words, she made an effort to do so, but although there was a tendency for the letters to come right, they were seldom placed together quite correctly.

After discussing the different forms of aphasia, Dr. Wilks says that for the production of language three different processes are employed. First, the mind is impressed with ideas through the eye by means of written signs, that is, impressions formed on the retina pass through their own perceptive centres to the central hemispheres, where they are further developed or compounded with other conceptions. Secondly, impressions made on the ear by sounds are carried through the auditory perceptive centres, also to the hemispheres. Thirdly, the sound made by the master must be imitated by the child until the latter can use its organs of speech in a particular way. Thus it is possible for a person to read French or German, who cannot speak either of these languages or understand them when spoken; or he may be able to read them and understand them, but not to speak them. This compound of language made up of spoken, auditory, and visual signs, may be thoroughly broken up for instance, the faculty of speaking may be virtually paralyzed in an exhausted state of the nervous system, or in a less degree from want of food or overexertion. Even in health we lose the memory of words; we may not be able to speak them or write them, but we recognize them when written or spoken.

Dr. Wilks combats the very prevalent opinion that when we are thinking we are really mentally speaking, for he says, it may be true that in thinking, we must conjure up an appropriate sign, but it does not follow that this is by mental or imaginary speech. Thus a person may be able to recall to memory a passage in a language which he has acquired only by reading. A child too may think of the picture of the dog in its lesson book without necessarily attaching to it any spoken, auditory, or visual signs, and the child could then go on to think about various actions of the dog, as running, eating, etc., without the idea of words or any artificial signs. Mental operations are, moreover, frequently very active in the deaf and dumb, and yet they are, of course, without

any comprehension of spoken language. It is doubtful, however, whether any really deep intellectual processes can go on without language, and certainly few individuals can accurately think out a subject without speaking or writing, showing that the ideas must have expression in form.

Dr. Wilks then goes on to speak of the duality of the brain. This, he thinks, is proved to exist by the fact that speech may sometimes be regained after it has been lost. With Broca, he places the organ of speech on one side of the brain. Dr. Moxon holds that the localization of the organ of speech on one side of the brain is entirely owing to the education of the two different sides, and that when language is regained, after having been lost, the right side has been in some way educated to take the place of the left. Other facts in favour of the theory of the duality of the brain are the occurrence of hemiplegia from inflammation of one hemisphere, of convulsions of one side from a lateral source of irritation, and of hysterical hemiplegia, in which there is a want of will to move the muscles on one side of the body, whilst the will is good to move those of the other side.

We regret that the space at our disposal has not permitted us to give a fuller abstract of Dr. Wilks's views on the subject of duality of the brain, as well as on that of aphasia. These will be found to be very fully and ably discussed.

A Case of Hysteria, with Remarks on Diagnosis and Treatment.—The symptoms in this case indicated that there was no actual lesion in any part of the nervous tract standing in the way of a voluntary act, but that rather the voluntary power itself was at fault; it was in fact ideal rather than real paralysis. Under these circumstances the writer says: "As the object was to arouse the dormant will, and not to treat the paralysis as a physical reality, it was considered that medicine alone would be useless, or worse than useless, but that rather those means should be used which could act directly on the will—in a word, moral means. The patient was informed that an effort on her part was essentially necessary for the cure, for if she did not exert herself she would soon become irremediably paralyzed." Dr. Wilks further says, that the physician is very often responsible for the continuance of symptoms in cases of hysteria, and refers to cases in which cure has taken place after all treatment has been given up. He also expresses the opinion that the chances of recovery for an hysterical patient are greater in a hospital than in private practice, where she is often surrounded by sympathizing, but injudicious friends.

Contraction and Spasmodic Affection of the Limbs.—After alluding to the very different opinions held with regard to the immediate cause of spasmodic affections of the limbs, Dr. Wilks reports two cases of intermittent tetany similar to the cases reported in a former volume of these reports by Dr. Moxon, and noticed in the number of this Journal for July, 1870, p. 192.

Contraction of the Limbs and Sclerosis of the Spinal Cord.—Dr. Wilks is disposed to believe that the causes of such contraction are to be found in the nerves of the limb itself; that the nerves being in some way disconnected with their centre undergo a degenerative change throughout their course. If this be true, he says, it is also probable that in the rigidity accompanying recent paralysis, the nerve may be acutely involved, which affords an explanation of the pain which occurs on moving the limb. Sclerosis of the cord has been said by Charcot to exist in all cases in which permanent contraction of the limb is a symptom. Dr. Wilks, however, refers to a case in which, during life, the patient lay in bed for many months with his legs drawn up, his heels to the nates, and knees to the abdomen, and had great pain in moving them. He also had extreme excito-motility. In this case the membranes of the spinal cord were

found to be enormously thickened, but the medulla itself was not affected except immediately on its surface.

A case of *Tremour* and one of *Paralysis Agitans* are also reported, with remarks, after which the author passes to the subject of *Local Paralytic Affections*, of which the histories of five cases are detailed.

Ataxia is apparently considered by Dr. Wilks, to a certain extent, a curable affection, and one which in all cases admits of relief. The treatment which he recommends is the application of the constant-galvanic current, the poles being placed in the neck and lower part of the spine. Patients in whom this treatment has been resorted to early, generally recover.

The reports of three cases of *Syphilitic Epilepsy* close Dr. Wilks's very valuable paper. In the remarks that precede these, he says that, in epilepsy dependent upon syphilitic disease of the brain, the paroxysms occur in rapid succession, and coma may exist in the intervals; there may also be convulsions without loss of consciousness, or the attack may be accompanied by paralysis of one side. In cases in which a succession of fits occur there is reason to refer them to syphiloma between the membranes and the brain; but if this be situated in one hemisphere, the circulation causes the convulsion to be unilateral or predominant on one side, and to be followed by partial paralysis of that side.

Art. III. *A Case of Hæmatidrosis, or Bloody Sweat, Complicating Tetanus.* By SAMUEL WILKS, M.D., F.R.S.—The attack of tetanus, in the case reported by Dr. Wilks, seems to have been idiopathic, as no wound was discovered during life, and only a slight abrasion of the mucous membrane of the uterus, which was prolapsed, detected after death. The perspiration from the patient's forehead stained clean white cloths of a reddish colour. These were given to Dr. Stevenson for examination, who made the following report: "The supposed blood-stains from the sweat are extremely like iron-moulds. On incineration they yield to acids a little iron, and the ash is red from the presence of iron. The stains are quite insoluble in water and in alkaline solutions; in this respect they are totally different from the colouring matter of the blood in all its forms. I have been unable to procure any *coloured solution* or blood-corpuscles from the stains. They are, therefore, not blood. Possibly they may be derived from blood, because they contain *iron;* but just as probably they may have never *contained* hæmatin or hæmoglobin. As the unstained portions of the napkin turn tincture of guiacum blue, the guiacum test is inapplicable. The quantity of colouring matter must have been very small."

Although we are assured that all possible sources of error were guarded against, it seems to us not impossible that the cause of the turning of the tincture of guiac of a blue colour and the discolouration of the napkins by the perspiration may have been the same; in other words, that there may have been some foreign body or impurity in the cloths which were used.

In the cases of bloody sweat which have been recorded, mental emotion of a very depressing character has been a very prominent symptom. This was notably true of Charles IX., of France, who died two years after the massacre of St. Bartholomew, and who confessed to his surgeon, Ambrose Paré, that, whether asleep or awake, he had visions of murdered corpses before him, and, therefore, no doubt, died in great mental agony.

The subject of "bloody stigmata" is discussed in this paper, and the case of Louise Lateau is referred to in full. Certain of the lower animals, says Dr. Wilks, have the power to exude from their skin a reddish fluid. Thus the hippopotamus, soon after its arrival in England, was observed, after taking its

bath, to be covered with a red secretion, and the flamingo threw up a red liquid from the stomach, which has been found by Mr. Lowne to contain red corpuscles, and which is probably used for nursing its young. This may, Mr. Lowne says, account for the old fable of the pelican feeding its young with its blood, supposing that the flamingo was mistaken for the pelican.

Art. IV. *Toxicological Cases.* By THOMAS STEVENSON, M.D.—Four cases are reported in this paper. In the first case a man, aged 21 years, swallowed three fluidounces of nitric acid with suicidal intent. Death took place seventeen hours after the poison was taken. The post-mortem appearances were those usually observed in poisoning by the mineral acids.

The patient, in the second case, drank about a teaspoonful of strong liquor ammoniæ. Death resulted, it was thought, from some obstruction of the respiratory apparatus, and was sudden. The danger arising from swallowing ammonia is greater from its action on the larynx than from that on the stomach.

In the third case, three-sixteenths of a grain of atropia were administered by mistake to a child six years old. The usual symptoms of atropia poisoning followed. Four and a quarter hours after the poison was taken, one-ninth of a grain of acetate of morphia was injected subcutaneously. In twenty hours the child had quite recovered from the effects of the alkaloid. But Dr. Stevenson is not disposed to attribute any share in the recovery to the action of the morphia. He is inclined to think, however, that we have an antidote for atropia in physostigma.

The patients in the last case reported by Dr. Stevenson were two children who were poisoned by eating mushrooms. One of them had partaken freely of the fungi, and died; the other only ate one, and recovered. The symptoms were vomiting, violent pains in the head and abdomen, restlessness, subsultus, and thirst. The legs were also drawn up towards the abdomen. After death, in the fatal case, the stomach and the whole of the small intestines were contracted and empty, and the mucous membrane congested throughout the whole of the alimentary canal. The liver was fatty.

Art. V. *Laboratory Notes.* By THOMAS STEVENSON, M.D.—In two specimens of milky fluids removed by paracentesis from the abdomen, the turbidity appeared to be due to very fine suspended particles of albuminous matter, which were not dissolved by ether, even when the fluids had been previously treated with acids and alkalies. Dr. Stevenson, believing the question, whether the so-called fatty heart consists of a real conversion of the muscular fibre into fat, to be an open one, has examined the mottled interior of the ventricular walls of two such hearts, with the following results:—

	Fat per cent.
Normal heart	0.68
Fatty heart, No. 1 . . .	1.54
Fatty heart, No. 2. . . .	3.83

It thus appears that there is a real conversion, to a certain extent, of muscular fibre into true oily fat, soluble in ether.

Some salivary calculi subjected to analysis were found to contain, in one instance, 78.13 per cent. of the mixed phosphates of lime and magnesia, and 21.87 per cent. of organic matter; in the other the calculus consisted, besides organic matter, of earthy phosphates and carbonates, the carbonates, however, predominating over the phosphates.

Dr. Stevenson finally discusses the "Effects of Drinking Water Contaminated with Zinc." He is inclined to think, in opposition to Fonssagrives, that water

may become so contaminated when kept in zinc or galvanized iron vessels as to be, when drunk, seriously injurious to health. Under certain conditions the water acts so energetically upon zinc and upon galvanized iron as to become milky from the large amount of the metal held in suspension in it.

Art. VI. *Remarks on Cerebral Disease having its Origin in Syphilis.* By G. OWEN REES, M.D., F.R.S.—Dr. Rees thinks that cerebral disease of syphilitic origin may generally be recognized even in cases where the possibility of infection is denied by the patient by attention to the following points. The paralytic seizure when dependent upon syphilis is generally the immediate result of some violent exertion, or some long-continued muscular effort carried on to fatigue, and the collapse is often so great as to threaten immediate dissolution. The hemiplegic and the paraplegic symptoms are peculiar in character, there being marked irregularity in loss of motor power, and of sensation as affecting opposite sides of the body. Pain in the head and tenderness of the scalp are scarcely ever wanting. Aphonia has been observed in many cases in the early stage. Whenever these symptoms are presented by a patient, the writer of the paper thinks a course of mercurial treatment should be at once begun. "The power of the iodide of potassium," he says, "in absorbing homologous growths, such as those observed on bone, is beyond doubt, and probably we may confidently trust to its use in all cases of new growth of connective tissue, where the mere proliferation of cells has not been succeeded by fibrillation and an approach to retrograde metamorphosis. These changes occurring, the formation of gummata seems to place such tumours beyond the power of the iodide of potassium, while mercury appears to be their especial antagonist.

Art. IX. *A Case of Simple Stenosis of the Œsophagus, followed by Epithelioma.* By C. HILTON FAGGE, M.D.—In the case reported by Dr. Fagge there was reason to believe, as the title of the paper implies, that the epithelioma was probably of comparatively recent development, and that the dysphagia which had been a marked symptom for many years before the death of the patient was caused by the narrowing of the cardiac end of the œsophagus. He was 84 years old at the time of his death, and had had for upwards of forty years a difficulty in swallowing food, which he thought originated in the habit of eating fast. When it first came on it seemed to him as if, when he swallowed anything, the wind met it and forced it back. He used to wash down solid food, which was always divided into very small portions, with liquids. He was not known to have suffered from sickness, nor did he ruminate nor bring up his food in pellets. After death the upper part of the œsophagus was found to be rather less than an inch in diameter. About four and a half inches from the cardia it became slightly dilated, its diameter reaching one and a half inch. About two inches lower it became narrower, at first rapidly, afterwards more gradually. The narrowest part of all was at the termination of the œsophagus in the stomach. The calibre was there so small that an ordinary lead pencil was firmly grasped by the gullet. At its widest part the walls of the œsophagus presented a large irregular opening, two inches in length, which was evidently in part the result of ulceration, but which may also in part have been caused by injury during removal from the body. On one side the lung was adherent; there was also close to the outside of the gullet a lymphatic gland, which was the seat of epithelioma. A microscopic examination of the specimen was made by Mr. Howse, who reported that it presented the ordinary appearances of epithelioma. Dr. Fagge also gives the history and post-mortem appearances of a case reported by Dr. Wilks, in the seventeenth volume of the

"Pathological Transactions," in which many of the same symptoms occurred as in the case which forms the nucleus of his paper. Dr. Wilks's case was found, however, to be simply one of stenosis, which, there was reason to believe, was congenital. The author also refers to some cases reported by Dr. Graves, and by Sir Everard Home, in which similar symptoms existed. These were attributed to spasmodic stricture of the œsophagus, but there can be little doubt, Dr. Fagge says, that they were due to stenosis. In regard to the so-called dysphagia lusoria, the writer says there is no good reason to believe that it is produced by any peculiarity in the course of the right subclavian artery. Two plates accompany this communication.

Art. X. *Clinical Cases.* By S. O. Habershon, M.D.—These cases, the histories of which are detailed in this paper, are as follows:—

1. Cancerous Disease; Paralysis of the Ninth Nerve.
2. Paraplegia; Strumous Tumour in the Spinal Cord.
3. Rheumatism; Cardiac Inflammation; Disease of the Brain from Embolism.
4. Disease of the Aortic Valves; Ulceration of the Valves; Acute Cerebro-Spinal Meningitis.
5. Cardiac Disease; Imperfect Ventricular Septum, patient aged 54.

The patient with cancerous disease was a woman, aged 52, who five years previously to coming under Dr. Habershon's care, had a small tumour of the breast, which she was advised by Mr. Birkett to have removed. There was at the time of admission a lateral curvature of the spine, so that the ribs on the right side were on a lower plane than the crest of the ilium. On asking the patient to protrude her tongue, the lip was sent to the right side, and that side of the tongue was quite flaccid and wrinkled; the left side, however, felt fleshy and firm. This was associated with inability to manage the food, with paralysis of the sterno-hyoid muscle on the right side, and with slight impairment of the right facial nerve.

At the post-mortem examination, masses of cerebriform cancer were found within the cranium. One such mass was situated on the basilar process, more on the right than the left side, and extending into the right temporal bone, and this involved the eighth and ninth pair of nerves, and reached up to the internal auditory foramen, one side of which was implicated. The muscles supplied by the ninth were yellowish and wasted. The third and fourth dorsal vertebræ were affected with cancerous growth, as were also both ilia.

A case presenting some similar symptoms, which was under Dr. Habershon's care, is also referred to in this paper.

The patient with paraplegia was a woman, aged 40, who, in addition, presented the signs and symptoms of phthisis pulmonalis. About a year before coming under observation, she began to experience aching pains in both legs, but in the right more than in the left; this was followed in the course of a week by numbness and a sensation of "pins and needles" in the feet; at the same time, her legs began to grow weak, and she found herself unable to get up and down stairs without assistance. The numbness gradually crept upwards, and the parts affected in this way felt cold to the patient. In the act of walking, her legs were spasmodically drawn up, and the foot thrown out. On moving the legs, she experienced acute pains in the knees and shins, which subsided when the limbs were at rest. Sensation was so much impaired, that she was quite unable to perceive a gentle touch. Pinching and pressure excited reflex action, causing piercing pains in the leg and foot.

The paralysis gradually became more marked, and the patient finally sank from exhaustion produced by erysipelas and a bedsore.

At the autopsy, in addition to disease of the lungs and ovaries, a round tubercle of the size of a small cherry was found in the lower part of the dorsal region. This quite took the place of the tissue of the cord for the length it occupied, so that scarcely any trace of nervous tissue could be seen about it.

In the third case, the patient, who was also a woman, and who had been for some time a sufferer from heart disease, was, when admitted into Guy's Hospital, paralyzed upon the right side. The paralysis was attended with aphasia. Its dependence upon embolism of the left middle cerebral artery was diagnosticated during life, and proved after death.

In the case of disease of the aortic valves, acute cerebro-spinal meningitis supervened, it was thought by Dr. Habershon in consequence of the changes in the composition of the blood produced by the ulceration of the valves. Although pus was found beneath the arachnoid, about the pons, the diamond-shaped space, the cerebellum, and all down the spinal cord on its dorsal surface, the symptoms were not indicative of the disease, for the patient is said merely to have been strange in his manner, to have walked about the ward at night, and two days before his death to have complained of severe pain in the back of his head and neck.

The chief interest in the remaining case reported by Dr. Habershon is the fact that the patient had lived in apparently perfect health for fifty-nine years with an imperfect septum between the right and left ventricles of the heart. During the life of the patient, the malformation was not suspected and the disease under which the patient was labouring was in consequence of the physical signs thought to be dependent upon mitral regurgitation. The bruit was, however, very distinct to the right of the sternum, and there was unusual oppression at the scrobiculus cordis. After death, the communication between the ventricles was found immediately below the aortic valves, at the base of the heart; it was about a quarter of an inch in diameter, circular in form; on the left side it had a rough atheromatous margin; on the right there was a funnel-shaped opening behind the curtain of the tricuspid, and small ridges of fibrin were observed for an inch upon the valve, as if the direction of blood had been from the left to the right.

Art. XI. *A Record of Observations of Temperature during Parturition and in the Puerperal State.* Presented by J. BRAXTON HICKS, F.R.S. Arranged by F. S. CAREY, Esq., Senior Obstetric Resident.—Mr. Hicks has not attempted to draw any conclusions from observations of temperature which he has presented in this paper. He has put them on record simply because, to use his own words, "it is impossible to say how far any rise of temperature may indicate danger, unless we are aware what is the natural temperature; or within what limits we may expect it to vary from slight causes in a natural state of convalescence." In cases in which the labour was perfectly natural and not tedious, the temperature of the patient was scarcely affected by it. There was in some instances a rise of temperature on the third day, but this was by no means true of all the cases. The temperature, on the other hand, was observed to be very high immediately after the completion of labour in three primiparæ who were delivered by the forceps after unusually protracted labours, and also in one patient in whom premature labour was induced by introducing a catheter into the uterus and injecting four fluidounces of water between the uterus and the membranes and leaving the catheter in.

Art. XIV. *An Account of the Recent Additions made to the Hospital*

Buildings. By J. C. Steele, M.D.—Dr. Steele gives in this paper a very full and interesting account of the plan of a new wing which was completed last June, and which increases the capacity of the hospital by at least 150 beds. The method of ventilating and heating the new building are also explained and seem to be in accordance with the most advanced ideas on these points. Three diagrams accompany this paper.

Art. XV. *Numerical Analysis of the Patients Treated in Guy's Hospital during* 1870. By J. C. Steele, M.D.—From this paper we learn that the whole number of patients treated at Guy's Hospital during the year 1870 was 81,771, of whom 5123 were under treatment in the wards of the hospital, and 76,648 came under the category of out patients. The daily sick population of the Hospital was 486. The death-rate for the year was 10.76. The percentage is higher than the average, simply because, while the number of admissions has been small, the character of the injuries or surgical complaints of those admitted has been severe. The average stay of each patient, whether living or dying, in the Hospital, has amounted to 36.92 days. Of the 5123 patients treated in the wards, 1673 are reported cured; 2507 relieved; 396 unrelieved; 498 died, and 499 remained at the close of the year under treatment. The list of accidents admitted into the surgical wards during the year amounted to 827, and the deaths resulting therefrom were 91; 61 amputations were performed; 31 times in consequence directly of accidental injury; 24 deaths were attributed to pyæmia, 8 of which occurred after operations. The following analysis of 7312 cases of death from pyæmia occurring in seventeen years, or between 1854 and 1871, will be interesting to our readers:—

Pyæmia after surgical operations:		
After amputation of limbs for compound fracture and laceration		56
After amputation of limbs for disease, excluding injury		37
After other operations for injury		3
After operations for disease, excluding amputations		30
		126
Pyæmia without surgical operation:		
After compound fracture of limbs		29
After other external injuries		30
Consequent on disease		109
Apparently idiopathic		18
		186
		312

J. H. H.

Art. XIX.—*Medico-Chirurgical Transactions.* Published by the Royal Medical and Chirurgical Society of London. Vol. LIV. 8vo. pp. xlii., 358. London: Longmans, Green, Reader, & Dyer, 1871.

According to our custom, we herewith present our readers with an analysis of the papers contained in the last volume of the *Transactions of the Royal Medical and Chirurgical Society.* The volume fully maintains the reputation which the series has gained for itself, and will be found, we think, not an unworthy successor to the earlier volumes. The Society numbers among its members the most distinguished physicians and surgeons of Great Britain, and

therefore contributions to these *Transactions* are derived from a greater variety of sources than are those of any other medical series with which we are acquainted.

We shall first call attention to the medical papers in this volume.

Art. I. *On the Electrolytic Treatment of Hydatid Tumours of the Liver, with an Addendum on Simple Acupuncture.* By C. HILTON FAGGE, M.D., F.R.C.P., etc., and ARTHUR E. DURHAM, F.R.C.S.—It will be recollected that Dr. Althaus recommends, in his work on the Electrolytic Treatment of Tumours and other Surgical Diseases, the application of electrolysis to the treatment of hydatid tumours. Dr. Fagge and Mr. Durham appear to have been the first to carry this suggestion into effect in the human subject. In this paper they give details of eight cases of hydatid disease of the liver in which the operation was performed. This number, these gentlemen say, is as yet insufficient to enable them to state with certainty that the operation will invariably cure the disease, but up to the time of writing the paper they have not met with a single instance of failure. The results of the treatment are stated to be as follows: In three cases the tumour disappeared so entirely that no trace of it could be discovered. In three cases some ill-defined fulness existed when the patients were last seen, and in another case, in which there had been two distinct tumours, a mass the size of a walnut could be felt when the patient was last examined. In the remaining case complete recovery took place at the end of nine months.

As regards the production of pain and constitutional disturbance, the results of the operation appear to have varied considerably in different cases. In most instances more or less pain came on after a time when the effects of the chloroform, which was always used, had passed off. The patient, if a child, became fretful. There was always some febrile disturbance, the temperature rising to 99.5°, 101°, or even 102.6°. By the third or fourth day, however, all these symptoms generally subsided, but in one instance, the temperature remained high for at least nineteen days after the operation.

The operation was performed in general accordance with the directions given by Dr. Althaus for the cure of other diseases by the electrolytic method, his electrolytic needles being passed into the tumour, one or two inches apart; they were then attached to two metallic wires, both connected with the negative pole of a galvanic battery of ten cells. A moistened sponge formed the termination of the positive pole, and this was placed on the patient's skin at a little distance from the points of entrance of the needles. Its position was changed from time to time during the operation. The current was allowed to pass for about ten minutes in some cases and in others for a somewhat longer period. At the end of this time the needles were gently withdrawn and the seats of puncture were covered with adhesive plaster.

As regards the *modus operandi* of electrolysis in the cure of hydatid tumours there is at present room for doubt.

"We have, the author says, already stated that in several instances we have been able to detect the presence of fluid in the peritoneal (and in one instance, even in the pleural) cavity very soon after the operation, and that we believe this to have resulted from the passage of part of the hydatid fluid through the apertures made by the needles. It has occurred to us that this may perhaps be an essential element in the success of the operation. It is well known that simple tapping of an hydatid cyst, is often sufficient to cure the disease; and it may be that electrolysis is, as it were, in effect a kind of subcutaneous tapping, with effusion of the cyst fluid into a serous cavity. We have even thought that the hydrogen gas set free within the tumour by the electrolytic action of the current accumulating in the upper part of the cyst, may cause a pressure suf-

ficient to *force* out the fluid, either by the side of the needles during the operation or subsequently through the apertures made by them. If this be not the case, an equally successful result might possibly be attained by simple acupuncture of an hydatid tumour."

In the addendum to the paper, a case is reported in which a hydatid cyst of considerable size was treated by simple acupuncture. The patient, a girl 9½ years of age, having been placed under the influence of chloroform, two fine gilt needles were introduced into the interior of the tumour at two of its most prominent parts, they were left in for five minutes, when it was found that one of them had slipped out of the tumour. Both of them were then withdrawn. Immediately afterwards the abdomen felt less tense than before. The temperature began to rise soon after the operation, and by 2 P.M. on the following day had reached 102°. Fluctuations were noted for some days afterwards, the highest point observed being that of 102.6° on the fourth day from the performance of the operation. In other respects the child appeared to be as well as usual. The tumour underwent gradual but progressive diminution in size, but unfortunately the child passed from under the observation of Dr. Fagge, and the result is not certainly known although there is every reason to believe that it was favourable.

Dr. Fagge and Mr. Durham admit that it is generally believed that hydatid fluid exerts an irritant action on serous membranes, and that many instances have been recorded in which death has been rapidly brought about by the rupture of hydatid cysts and the pouring out of their contents into the peritoneal cavity. But they say it is probable in these cases that death is due to the escape of secondary cysts and scolices, which are much more capable of producing severe irritation than the chemically inert hydatid fluid, and which are not likely to pass through the minute openings made by the needles in the operation of acupuncture or electrolysis.

Art. II. *A Third Communication on the Endemic Hæmaturia of the South-eastern Coast of Africa, with Remarks on the Topical Medication of the Bladder*, by John Harley, M.D., Lond., F.R.C.P.—Since Dr. Harley presented his last communication to the Society—he "has had almost daily opportunity throughout nearly a year of watching the progress of the disease," and is consequently able to give more precise information respecting certain features of this interesting malady. His observations have reference, *first* to the prostatic form of the disease and its treatment; and, *secondly*, to the structure and development of the parasite. In the prostatic form of the disease, there is very apt to be—

"An intermittent discharge, by the urethra, of venous blood, never exceeding a teaspoonful, and always mixed with urine, recurring after variable intervals of from two to fourteen days, and continuing for several days in succession. Excepting a cloudiness of the urine from the presence of free mucus, which on settling formed a russet or drab-coloured fluffy deposit, the morbid appearances were confined to the last tablespoonful of urine. With this was discharged a variable amount of blood and the branched mucous or blood casts of the burrows of the parasites so characteristic of the disease. The eggs were chiefly contained in these casts, but numbers were always to be found free in the urine."

Dr. Harley at first used in the treatment of the disease remedies which are eliminated by the kidneys, but failing to effect any decided improvement in the condition of his patients he substituted local for general treatment. This consisted in the injection of various substances in the form of solution or emulsion into the bladder. The chief of these were iodide of potassium, quassia, wormwood and oil of male fern. With the last three no decided results were ob-

tained, but the iodide of potassium when used in this way seemed to have valuable parasiticide qualities. A solution, Dr. Harley says, containing five grains of this salt in the ounce fails to produce the least irritation when placed in the eye, but if a healthy leech be dropped into it, the animal immediately begins to writhe, the movements then become arrested, and death occurs in the course of an hour or so. He began by injecting the following every other morning. ℞. Potassii iodidi gr. x; infusi quassiæ (℥iv ad f℥xx) ℥v. Solve et fiat injectio. The iodide of potassium was gradually increased to gr. xxx, and this treatment was continued for two months. The iodine injection was usually retained for three hours, iodine being at the end of that time easily detected in the urine. The catarrhal symptoms and iodine taste caused by these large doses of the salt were at first rather severe, but the patient gradually became accustomed to them, and ceased to make complaints. The iodide of potassium never produced the least urethral or vesical irritation. Speaking of the results of this topical treatment, Dr. Harley says:—

"The first effect of the injection was to bring away a great quantity of the branched mucous casts of the tunnels formed by the parasite with hosts of imbedded eggs. Some of these casts measured more than one-fourth of an inch in diameter. After a few weeks the casts lose their solid cylindrical character, and were ultimately replaced by long skin-like membranes, destitute of structure, and only here and there thickened by the usual mucous corpuscles. These casts seemed to be formed by exudation from the walls of the emptied tunnels. They were almost destitute of ova, and some of them were three-fourths of an inch long. At the same time free ova nearly disappeared from the urine. * * Thus it appears conclusively that the topical treatment was effectual in clearing away the accumulated products of the parasite, in securing their expulsion as fast as they were formed, and ultimately in destroying the adult parasite."

Several pages are devoted to the discussion of the structure and development of the embryo of the distomum hæmatobium, but in consequence of want of space, we can only refer those of our readers who are specially interested in the subject of the parasite, to this part of Dr. Harley's paper.

There is reason to believe that the animal gains access to the body through the skin. It is stated that every colonist of Natal is attacked during the first year or two of his residence in the country with large indolent sores, which spread like syphilitic ulcers and leave deep scars. There is reason to believe that these are due to the irritation excited by the animal implanting its ova in the skin of the individual while bathing. The animal may, however, enter the body through the mucous membrane of the stomach or rectum, or by the more direct route of the urethra.

Art. III. *On Temporary Glycosuria as a Sequel to the Cholera*, by WILLIAM SEDGWICK, M.R.C.S.

Art. IV. *On the Duration of Phthisis Pulmonalis, and on certain Conditions which influence it*, by CHARLES THEODORE WILLIAMS, M.D.

Abstracts of these papers will be found in the No. of this Journal for April, 1871 (pages 567 and 558).

Art. VI. *On Neuritis of the Brachial Plexus*, by JULIUS ALTHAUS, M.D., M.R.C.P., etc.—The patient whose case is reported in this communication presented a complete paralysis of motion and sensation of the whole of the right upper extremity. The muscles of the arm were not wasted, and the temperature of the limb was not lower than that of the left arm. There was an absence of electro-muscular sensibility to the induced current, but when a powerful continuous current from Daniell's battery (sixty cells) was applied to the arm the patient exclaimed that she felt sensations of heat, pricking and tingling in

the arm and fingers; and the muscles, more especially the flexor muscles of the fingers responded to the current by sluggish, yet plainly perceptible, contractions. The daily application of the continuous current to the paralyzed extremity formed the only treatment in the case. The result is stated to have been exceedingly satisfactory. After four applications sensation returned in the paralyzed limb, so that the patient was again able to perceive touch, pricking, pinching, etc., just as well as before the commencement of the affection. The paralysis of motion proved more stubborn, but after seven applications the girl was able to move her fingers. A fortnight later she could flex and extend the wrist. The treatment was suspended for six weeks, during which time no improvement took place. The continuous current was then reapplied and continued until recovery was complete.

Dr. Althaus is disposed to attribute the recovery in this and similar cases partly to the power which the continuous current has of preventing granular degeneration of the muscles by making them contract. It must also have the property of re-establishing the conductivity of the sentient as well as of the motor fibres of paralyzed parts; although we are ignorant as yet of the manner in which this is done.

The differential diagnosis in this case is very carefully discussed by Dr. Althaus, who gives very good reasons for considering it one of Neuritis of the Brachial Plexus. "This plexus imparts," he says, "motive and sentient power to the whole upper extremity; it animates the rhomboid muscles, the serratus anticus, the supra and infra spinatus, the subscapularis, the anterior portion of the latissimus dorsi, and all the flexors and extensors, pronators and supinators of the arm and hand. Not one of these muscles had escaped the paralyzing influence, while muscles in their immediate neighbourhood, which are animated by a different set of nerves, had remained in perfect condition. Moreover the skin of the back of the head, the neck, and the ear, had never lost its sensibility. The absence of electro-muscular sensibility, the paralysis of sensation, the limitation of the paralysis to an upper extremity render it improbable that the patient was suffering from cerebral disease, and the same symptoms enabled him to exclude spinal disease from consideration.

Art. VII. *On Sporadic Cretinism occurring in England*, by C. Hilton Fagge, M.D., F.R.C.P., etc.—Sporadic cretinism, Dr. Fagge says, differs from the more ordinary form of cretinism in the absence of any enlargement of the thyroid body, and in the presence of fatty symmetrical tumours, one of which lies beneath the skin of the neck, on each side, just external to the sterno-cleido-mastoid muscle. The affection is not necessarily congenital. It may arise as late as the eighth year in a subject previously healthy and well-developed. It is not inherited syphilis, nor is it related either to rickets or scrofula. It has been supposed in certain cases to have resulted from one or both of the parents having been intoxicated at the time of procreation; but this explanation Dr. Fagge thinks does not hold good in all cases, even for those which are congenital.

The sufferers from sporadic cretinism are generally of a mild and tranquil disposition, and do not possess the mischievous tendencies of idiots, with whom they are very apt to be associated. As goitre also exists in the same parts of England, Dr. Fagge believes that there is a certain antagonism between it and cretinism, and that where a large goitre exists, it may possibly have the power of protecting against the more severe effects of the endemic influence, and thus that cretinism is associated with an enormous bronchocele only when the exciting cause is present in a very intense degree.

Art. XIII. *On Dextral Pre-eminence*, by WILLIAM OGLE, M.D. OXON, F.R.C.P.—This is one of the ablest and best-written papers in the volume. After briefly reviewing the literature of the subject, Dr. Ogle gives his reasons for believing that right-handedness depends on some predominance of the left brain, and left-handedness, when it occurs, on a transposition of this structural peculiarity, whatever it may be. If the two hemispheres be compared, the left will be found a little larger, and in the frontal lobes, especially, the left convolutions will be observed to be much more complicated. In the few instances in which he has had the opportunity of examining the brains of left-handed persons, the reverse of this has been found to be the case. The brains of monkeys possess the same peculiarities, and Dr. Ogle has satisfied himself by careful observation at the London Zoological Gardens, that the majority of monkeys are right-handed, and that a very small proportion (only three out of twenty-three), are left-handed. The parrot, too, will use its right leg to support itself in preference to its left. Thus of eighty-six parrots, sixty-three invariably supported themselves on the right leg, while the remaining twenty-three as invariably perched on the left one.

It might be objected to Dr. Ogle's view, that the greater development of the left brain may be the consequence of the increased use of the right side, and not its cause. But this disparity between the two hemispheres exists in fœtal life, and Gratiolet has asserted that the convolution of the left frontal lobe appears earlier than the corresponding convolution of the right. Besides which, it is clear that some anatomical difference between the two sides must precede the right-handedness, and this difference must be somewhere in the brain, for the author shows very conclusively in the first part of his paper, that it cannot be referred to a conventual agreement made between the members of a community and handed down by educational influence from parent to child through successive generations. In a former paper he has expressed an opinion that the cause of the greater development of the left hemisphere is to be found in the difference of the blood supply to the two sides of the brain, and to this opinion he is disposed to adhere. He has found that the arteries which convey blood to the brain are, as a rule, somewhat larger on the left side than on the right, and that this rule apparently breaks down in the case of left-handed men. This increased size of the arteries of the left side, he thinks, cannot be regarded as a consequence of the increased use of the hemisphere to which they go, since the left vertebral artery is generally larger than the right, and yet from their uniting to form the basilar artery, they must be precisely alike so far as the *vis a fronte* goes. The course of the left carotid artery is, moreover, much less tortuous than that of the right, and this will of course secure a more liberal supply of blood to the corresponding hemisphere. In monkeys which have been shown to be the subjects of dextral pre-eminence, the arterial branchings appear to accord with those of man. "But in parrots," he says, "I find a striking corroboration of my hypothesis. In these birds there is very great variability in the arrangement of the right and left carotid arteries. In some few the two are of equal size. But this is the exception. In the great majority the arteries are unequal, and when this is the case, it is invariably the left carotid which is the larger of the two, as Merkel years ago pointed out. Sometimes, indeed, it would appear that the right carotid is rudimentary, or even entirely absent, and that the brain receives its whole blood supply from the vessels on the left. It is difficult to suppose that this is a mere coincidence; but if it be anything more, it renders the explanation I have advanced in the highest degree probable."

In favor of Dr. Ogle's view is the fact that in left-handed people paralysis of the left side is accompanied by loss of speech, which may be regarded as an indication that the left side of the brain is more developed in their bodies than the right.

Art. XIV. *On the Modifications produced in the Temperature of the Body by the Local Application of Cold and Heat*, by FREDERICK BARHAM NUNNELEY, M.D.—With the view of determining whether, under any circumstances, local applications of cold modify the temperature of the body generally, or of any part of it other than that on which they immediately act, Dr. Nunneley instituted a series of experiments, the result of which he thus sums up:—

"No change occurs in the temperature of any part of the body from the local application of cold, except that on which it immediately acts, unless the individual is in a state of more or less nervous exhaustion or there is decided coolness of the surrounding air.

"The local application of heat, exceeding in degree that of the blood, causes a slight rise of the general temperature, and a more considerable one of that of the extremities.

"If cold is now applied locally—as to any one of the extremities—the general temperature falls slightly and that of the extremities more notably below the natural standard; but both this fall and the previous rise with heat vary considerably, not, however, with the individual susceptibility to painful sensations of temperature, but rather in proportion to the facility with which the conditions regulating the normal balance of the bodily temperature can be disturbed.

"This change, unlike a reflex act, occurs comparatively, or even very slowly, and its amount is equal in all the extremities; hence, it is not, at any rate in many cases, a reflex action, taking place between homologous parts, as Brown-Séquard has described it." In conclusion he says, "the practical lesson which the facts teach us, is that partial chills *can* modify the temperature, and that a condition of depressed nervous energy is one in which an individual is particularly exposed to the effects of slight impressions of cold."

This paper is accompanied by two tables.

Art. XVI. *Report on Two Series of Cases in which Syphilis was communicated in the Practice of Vaccination*, by JONATHAN HUTCHINSON, F.R.C.S.—Few of our readers will, we think, after reading Mr. Hutchinson's admirable report, have much doubt that vaccination may sometimes be the means of inoculating syphilis. In the first series of cases, twelve persons, mostly young adults, were vaccinated from a healthy looking child. The progress of the vaccination appears to have been satisfactory in all, but during the eighth week indurated chancres were found on the arms of ten of them. They were all treated with mercury, which caused a rapid disappearance of the primary sores. Five months later, constitutional symptoms had been developed in four of the patients. The vaccinifer at the time of the operation had all the external evidence of health, but upon close examination two months later, was found to have condylomata. In the second series of cases, there were unquestionable symptoms of constitutional syphilis in nine children who had been vaccinated from the same patient. Suspicious symptoms occurred in six others, while several had escaped all infection. The vaccinifer, on examination, was found to be a fine, healthy looking child, but presented slight local symptoms indicative of inherited syphilis.

In both instances the child had been selected on account of its very healthy appearance, which had moreover attracted the attention of those who were vaccinated from it, and Mr. Hutchinson takes occasion to remark that "it is a mistake to suppose that syphilitic infants always present a withered, 'old-man-

like' aspect; in many instances, although manifesting specific local symptoms, they may grow well and remain plump and fat. This occurs, I believe, with especial frequency when the child does not suffer from a skin eruption, and under such circumstances condylomata are not unfrequently the only symptoms by which the existence of the taint is proved."

In the first series two of the patients wholly escaped, and in those in whom chancres occurred, nearly half of the vaccination punctures escaped. This immunity of some persons, while others suffered, and the absence of induration in some vaccination punctures, and the escape of some while others were infected in the same person, Mr. Hutchinson explains by supposing that the syphilitic virus and the vaccine virus were present in different fluids, and that it was possible to convey the one in all its vigor without necessarily conveying the other; and it is important to note that the two individuals who escaped were the two who were first vaccinated, since it is not improbable that the lymph which first flowed was unmixed with blood. It would thus seem that the inoculation of syphilis in these cases depended upon the admixture of the blood of the syphilitic infant with the lymph of the vaccine vesicle, and that it is quite possible to vaccinate successfully from a syphilitic infant in the stage of utmost potency, as regards its blood, without communicating syphilis, if the lymph be free from blood. The success or non-success of the vaccination, as vaccination, seems to have had nothing to do with the induration of the scars. Some scars in which the vaccination vesicle had not "taken," became indurated, whilst many punctures which did "take" escaped. The period of incubation seems to have been very nearly the same in all cases, and was a little more than five weeks.

The vaccinifers and the patients vaccinated from them were seen by a committee consisting of Dr. Samuel Wilks and Messrs. Savory, Gascoyen, and Thomas Smith, who reported that most of the latter presented unequivocal evidence of constitutional syphilis, and that there was no reason to doubt, from the appearances presented by the arms and from the history of the cases, that the disease had been conveyed by vaccination. J. H. H.

The remaining papers are particularly interesting to surgeons, and the first to which we shall invite our readers' attention, is

Art. V. *Three Cases of Injury to the Brain, with an Appendix of Cases*, by GEORGE WILLIAM CALLENDER, F.R.S., Surgeon to St. Bartholomew's Hospital.—This very interesting paper should be read in connection with those on "The Anatomy of Brain Shocks," which the author published in the third and fifth volumes of the *St. Bartholomew's Hospital Reports* and which were noticed in the numbers of this Journal for July, 1868, p. 225, and January, 1870, p. 191. In those papers Mr. Callender narrated cases in which convulsions were associated with disease of the meninges, and more especially with disease about the course of the middle cerebral arteries. Collecting now those cases, twenty-three in number, in which from lesion of the substance of the encephalon, there was one-sided paralysis, and adding to them three cases recently under the care of Sir James Paget (here given in detail), and eighty-three cases taken from the works of Bouillaud, Abercrombie, and Andral, and from the register of St. George's Hospital, a total of one hundred and nine cases of one-sided paralysis is obtained; these the author proceeds to analyze, and shows conclusively that convulsions particularly attend lesions of that part of the right hemisphere which is above the corpora striata. The figures are as follows:—

"Cases of side paralysis, ... 109
Cases in which rigidity or convulsions occur, . . . 46
Cases of right side paralysis, ... 48
Cases in which rigidity or convulsions occur, . . . 7
Cases of left side paralysis, ... 61
Cases in which rigidity or convulsions occur, . . . 39

"When examined with reference to the parts of the hemispheres involved in the disease or injury, we have the following results:—

"Cases in which parts above the corpora striata are affected:—

Left side, ... 37 cases.
Convulsions or rigidity in . . . . . 7 "
Right side, ... 47 "
Convulsions or rigidity in . . . . . 39 "

"Cases in which the corpora striata, including the optic thalami, are affected:—

Left side, ... 11 cases.
Convulsions or rigidity in . . . . . 0 "
Right side, ... 13[1] "
Convulsions or rigidity in . . . . . 0 " "

Four of the seven cases in which rigidity or convulsions attended lesions of the left hemisphere (right side paralysis), were instances of cerebral abscess, and the convulsions were probably caused by pressure transmitted to the right side of the brain.

An ingenious diagram is appended to Mr. Callender's paper, showing the seat of disease in the various cases included in his tables.

Art. VIII. *A Clinical Report on Xanthelasma Palpebrarum, and on its Significance as a Symptom*, by JONATHAN HUTCHINSON, F.R.C.S., etc.—A full abstract of this paper which is one of great interest and value, appeared in the quarterly summary of this Journal, in the number for July, 1871, pp. 264 *et seq.*

Art. IX. *On the Classification and Tabulation of Injuries and Surgical Operations in time of War*, by Deputy Inspector-General T. LONGMORE, C.B., Professor of Military Surgery in the Army Medical School, Netley.—In this paper Mr. Longmore describes and compares the several systems of classification and tabulation of gunshot injuries, etc., employed in the army medical departments of Great Britain, France, and the United States, and comes to the conclusion that the British system is, upon the whole, decidedly the most convenient and satisfactory. Mr. Longmore's remarks are deserving of careful study from all who are interested in military surgery; whether French and American surgeons will or will not be convinced by his arguments of the superiority of the British mode of registration, all will grant the justice of his concluding observation as to "the desirability of an agreement being come to in different countries for classifying the injuries of war, the surgical operations they lead to, and their results, on one general system; so that not only the statistics of different modes of treatment or operative interference in particular injuries in one country may be reliably compared with each other, but that the sphere of investigation and comparison may be extended, and the opportunity of making similar observations be afforded with regard to the statistics of all countries in common." In order to obtain such uniformity of registration, Mr. Longmore suggests that the subject should be referred to "an international committee of competent delegates, appointed either by the army medical de-

[1] One case of disease of the pons Varolii not included.

partments and governments of the countries respectively concerned, or selected by societies having a corresponding professional influence in them with that which the Royal Medical and Chirurgical Society holds" in England.

Art. X. *A Case of Bony Ankylosis of the Knee-joint treated by Subcutaneous Section of the Bone*, by LOUIS STROMEYER LITTLE, F.R.C.S., late Surgeon to the London Hospital. Communicated by T. B. CURLING, F.R.S., President.—This paper describes an operation which was performed in 1868, and which appears to have been suggested by the record of several cases of angular osseous ankylosis successfully treated by Brainard's method of subcutaneous drilling, and published by Dr. S. W. Gross in the number of this Journal for April, 1868 (pp. 360 *et seq*). Mr. Little employed a chisel and mallet (as long since suggested by Malgaigne), and the result of his operation was in every respect satisfactory.

Art. XI. *On the Removal of Tumours from Bone*, by Sir JAMES PAGET, Bart., D.C.L., F.R.S., etc.—The object of this short but valuable paper is to formularize and recommend a mode of treatment, which, though it has been occasionally resorted to in individual cases, has never been recognized as an established rule of practice. The mode of treatment in question is the enucleation, or simple extirpation, of non-malignant tumours growing in bone, rather than their removal by the more sweeping operations of resection or amputation. Six cases are detailed to show the feasibility and advantage of thus dealing with cartilaginous, bony, fibrous, and myeloid tumours, growing in bone; and the paper ends with certain suggestions as to diagnosis, which appear to us so valuable, that we quote them in full for the benefit of our readers.

"For cancerous and recurrent tumours, amputation or wide resection is generally advisable; for innocent tumours growing on bones, excision; for innocent tumours growing in bones, enucleation. . . . . .

"1. The tumour is probably cancerous if its growth commenced before puberty, or after middle age, unless it be a cartilaginous or bony tumour on a finger or toe, or near an articulation.

"2. If a tumour has existed on or in a bone for two or more years, and is still of doubtful nature, it is probably not cancerous or recurrent, and this probability increases with the increasing duration of the tumour.

"3. If a tumour on or in a bone has doubled or more than doubled its size in six months, and is not inflamed, it is probably cancerous or recurrent, and this probability is increased if, among the usual coincidences of rapid growth, the veins over the tumour have much enlarged, or the tumour have protruded far through ulcerated openings, and bleeds and profusely discharges ichor.

"4. If with any such tumour, not being inflamed, the lymphatics near it are enlarged, it is probably cancerous, and still more probably if the patient have lost weight and strength to amounts more than proportionate to the damage of health by pain, or fever, or other accident of the tumour.

"5. A tumour on the shaft of any bone but a phalanx is rarely innocent, and so are any but cartilaginous outgrowths on the pelvis, or any but the hard, bony tumours on the bones of the skull. . . . . .

"When the wall of the bone can be traced over the surface, or any part of the surface, of the tumour, its growth from within is nearly certain. And so it is when, on the surface of the tumour, portions of bone can be felt among portions of more yielding substance. General smoothness of surface is usually significant of a tumour growing within a bone and expanding it, unless in the case of cartilaginous tumours, which after growing within bones, have protruded through some part of their expanded walls. Pulsation in a non-cancerous tumour connected with bone, is a nearly certain sign of growth within bone, except in the case of certain specimens of myeloid epulis; and when such pulsation is felt, it is no indication that severe bleeding will ensue in the

removal of the tumour, for it is only derived from the arteries on the walls of the bone-cavity lodging the tumour."

Art. XII. *A fourth series of one hundred Cases of Ovariotomy, with Remarks on the Diagnosis of Uterine from Ovarian Tumours*, by T. Spencer Wells, F.R.C.S., Eng., etc.—A short abstract of this paper has already appeared in the Quarterly Summary of this journal, in the number for January, 1872 (p. 264). From Mr. Wells's remarks upon the diagnosis of ovarian from uterine tumours, we select the following:—

Visible enlargement of the abdomen, as ascertained by *inspection*, is more often general in cases of ovarian, and partial in those of uterine tumour, being in the latter confined in the early stages to the lower part of the abdomen. Flattening or even protrusion of the umbilicus is common in ovarian disease, but rarely accompanies uterine tumours, unless ascites is also present. Both forms of tumour may be seen to change their position with the posture of the patient, or, when she is recumbent, during inspiration and expiration; but nearly all uterine tumours, though visibly moving above, seem to be fixed below in the hypogastric region. When a recumbent patient tries to sit up, using only the muscles of the trunk in doing so, the recti bulge forwards in front of an ovarian tumour if this be non-adherent, or if, though adherent, it be a flaccid cyst; if, however, the tumour be uterine, the solid mass fixed centrally below the umbilicus interferes with the free action of the recti.

By *measurement*, the increase in the size of the abdomen is, in the case of an ovarian tumour, usually found to be greater on one side than on the other, but in the case of uterine growths, the increase is more often symmetrical. The distance from pubes to sternum is increased in both cases, but the increase in the distance from pubes to umbilicus is proportionately greater in the case of uterine tumours.

By *palpation*, the fluid part of an ovarian growth is commonly found to be in excess of its solid portion, the reverse being observable in the case of tumours involving the uterus. An ovarian tumour can usually be raised considerably, and the hand can sometimes be pressed backwards between the tumour and the pubes, almost to the brim of the pelvis; this can rarely be done in the case of a uterine growth.

By *auscultation*, distinct vascular murmurs of a tubular or vesicular character are not unfrequently observed in the case of uterine growths, but such a murmur in connection with an ovarian tumour is excessively rare.

The diagnostic marks to be observed in examination by the rectum and vagina are clearly indicated; a vascular thrill, like that of varicose aneurism, is regarded by Mr. Wells as peculiar to uterine growths, though an abnormal arterial impulse may be felt in either class of tumours.

Art. XV. *On a simple Method of removing Silver Wire employed in Cases of ununited Fracture*, by Francis Mason, F.R.C.S., etc.—Mr. Mason's plan is to pass a needle through the fragments and secure them with a loop, or figure-of-8, of silver wire, as in the operation for hare-lip, or as in Simpson's third method of acupressure. When the needle is withdrawn, the wire of course readily follows.

The present volume of the Transactions, which fully sustains the reputation of the series, terminates with an elaborate index, arranged on the same principles as the recently published general index to the first fifty-three volumes.

J. A., Jr.

ART. XX.—*Obstetrical Transactions.*

1. *Transactions of the Obstetrical Society of London.* Vol. XIII. For the year 1871. 8vo. pp. 325. London: Longmans, Green, & Co., 1872.
2. *Transactions of the Edinburgh Obstetrical Society*, Session 1868-69. 8vo. pp. 207. Vol. I. Edinburgh: Maclachlan & Stewart, 1870.

IT is impossible, in the space allowed for a review, to give a full account of the numerous papers contained in these two volumes. We shall, therefore, restrict ourselves to the selection of the more important.

The London Society has an "ordinary" fellowship of nearly 600, an income of some $3500, and holds ten sessions in the year. It is gradually accumulating a museum, library, and building-fund, and has a world-wide reputation for activity and energy. The volume of its transactions before us is much less in size than some that have preceded it, and opens with the Annual Meeting in January, at which Dr. Grailly Hewitt, the retiring president, delivered the Annual Address, and Dr. J. Braxton Hicks was chosen his successor.

Dr. Hewitt's address congratulates the Society upon its prosperity; contains obituary notices of several members deceased during the year; and reviews the work of the Society.

Spontaneous Rupture of an Ovarian Cyst; reported with specimen, by Dr. PROTHEROE SMITH.—Woman, single, aged 48, bowels obstinately constipated; fibroid tumour of uterus, and ovarian cyst diagnosed. Patient seized with severe abdominal pains, followed by vomiting, collapse, and death. . . . Flaccid ovarian cyst of right side, ruptured at a point covered by a globular fibroid tumour the size of a small orange, rising from left angle of fundus uteri. Cyst and pelvic viscera bound down by adhesions, the result of peritonitis. Dr. Smith exhibited specimen in evidence of the necessity of performing ovariotomy early under similar circumstances.

"Dr. Barnes observed that there was another mode of dealing with very early ovarian dropsy, from which good might be expected," and reported a case in which he had drawn off sixteen ounces of fluid, verified as ovarian, by means of the Aspirateur of Dieulafoy, the trocar being introduced by the vagina. "Improvement followed, and the uterus retracted to its natural position. In three weeks, the bladder distress, which existed prior to the operation, returned, and it was found that the uterus was again pushed forward by the tumour. It was tapped a second time, and six ounces of fluid came away; six drachms of tincture of iodine were thrown into the cyst. The patient was again better; no marked local symptoms followed. Some weeks later, a firm mass was felt in the place of the original swelling; and as the uterus was more forward than natural, it was resolved to puncture again, in the hope that, by letting off any little exudation in it, complete shrivelling of the cyst, and a cure, might be effected. But soon afterward, symptoms of irritative fever set in, attended latterly by jaundice, under which the patient sank after some weeks. Notwithstanding this issue, he thought the method deserved further clinical investigation.

Dr. Phillips referred to an American physician who advocated; and Dr. Wiltshire remarked that he believed had performed, successfully, the operation of ovariotomy through the posterior wall of the vagina. . . . [This physician was Prof. T. Gaillard Thomas, of the College of Physicians and Surgeons, New York, and the operation was performed February 6, 1870, terminating in entire success. See this Journal for April, 1870, page 387.]

Irritable Bladder in the Latter Months of Pregnancy.—Dr. W. S. PLAYFAIR refers to this sometimes distressing condition, and says :—

"I believe that this form of irritable bladder is mainly due to a distinct mechanical cause, viz., an oblique or transverse position of the fœtus in utero, and that it is easily and effectually cured by the simple expedient of altering the position of the child by means of external pressure on the uterus." He reports three cases in illustration, in one of which he several times altered the position of the fœtus to the great relief of the patient, the change not being permanent, as it appears to have been in the others, who were all equally benefited for the time, by the altered position of the fœtus.

Dr. Barnes proposed retaining the fœtus *in situ* by mechanical means. "Dr. Hewitt was at a loss to account for the irritation by the pressure of the fœtus on the bladder, because the head in the ordinary position would press quite as much as a shoulder, if not more, and that against the most irritable portion of the bladder, namely, the lower portion. He would suggest another explanation—the disturbance to the form of the bladder by the altered form of the uterus. It is well known that the vertical diameter of the bladder is increased, and its antero-posterior decreased, in the latter months of pregnancy; may not this arrangement be interfered with so as to produce the symptoms complained of?"

[As a matter of interest in this connection, we will state that one case of this form of irritability that came under our care, aborted at seven months, of an anencephalous fœtus. The lady was the mother of several children, and never had suffered to the same degree before, even upon the last days of full gestation.]

Remarks on Tables of Mortality after Obstetric Operations, by Drs. J. BRAXTON HICKS and J. J. PHILLIPS.—This paper, covering thirty pages, being itself condensed, the facts only can be stated, and deductions which were drawn from them. 1. *Craniotomy.* Smellie reports four fatal out of forty-four. Ramsbotham gives seven fatal cases. "Many of the conditions out of which it was endeavoured to rescue the patient were severe enough in themselves to be fatal, or at least seriously to endanger her life." "It will be observed that, out of the seven cases of Ramsbotham, rupture of the uterus occurred in two, and rupture of the vagina in two, and that in each of these cases the fatal accident had happened previous to any attempt at delivery. In two other cases, death resulted from sloughing, caused, as is evident from the history and the already decomposing fœtus, by the long continuance of unassisted labour. The remaining fatal case must be considered as altogether exceptional." Dr. Collins reports fifteen fatal cases. "When we analyze these fifteen cases of death after the employment of craniotomy, we are at a loss to find more than two whose fatal termination can with any show of probability be traced to the operation. In most of the cases, it will be admitted by all that death arose from the too long postponement of craniotomy." Dr. Robert Lee reports sixty-seven cases, eleven of them fatal. The remarks here have the same bearing as in the former instances, and so on to the end of the paper, including the results of the use of the forceps, and the employment of version; showing the unreliability of statistical tables in establishing the mortality to be anticipated from these operations, from the fact that the complications of delay, rupture of uterus, succession of instrumental expedients, etc., increase the mortality in the various cases, which cannot be properly associated under one head, either as forceps cases, craniotomy, or version. The whole paper is one well calculated to show the fallaciousness of the tables upon which the statistics of mortality, after the obstetric operations referred to, have been based. The remarks of Dr. Phillips at the close of the discussion which followed the reading of the

paper, will make a suitable explanatory ending to the imperfect sketch here given. He said that he was pleased to find that the general experience of the Society declared in favour of the belief expressed in the paper, that the mortality after obstetric operations in a large proportion of cases was wrongly attributed to the operations themselves. Probably many accoucheurs were influenced in delaying instrumental assistance by the high rate of mortality assigned to craniotomy, or the use of the forceps. The practical lessons were easily deduced, but with these, Dr. Hicks and himself were not on that occasion specially concerned; their chief object was to prove the unscientific manner in which the ordinary tables of mortality had been compiled, and to call attention to the wrong conclusions which had been drawn from them.

Extensive researches into the history of the Cæsarean operation, as performed in this country, have led the writer of this to a similar conclusion, the mortality being due less to the gravity of the operation than to all the accompanying circumstances of delay, fruitless attempts at instrumental delivery, etc.

A Case in which the entire Ovum was expelled at the seventh month, and the Child rescued alive, by Dr. JOHN BRUNTON.—The woman, aged 32, had previously given birth to four children, and on the occasion reported, which was in December, aborted of twins, the second being inclosed in the membranes for a period of time calculated at about fifteen minutes, before they were opened, when the child cried, and appeared in a better condition than the first: both died the following day, the second living much the longer of the two.

This report has a bearing upon the subject of intra-uterine fœtal independence, and especially upon the possibility of saving the child in placenta prævia after practising the method of Simpson in separating the placenta for the arrest of hemorrhage. Dr. Brunton states that in a case under his care the child was saved after the placenta had been removed and upon the bed for ten minutes.

The Vomiting of Pregnancy, its Causes and Treatment, by GRAILY HEWITT, M.D.—The purport of this article of nineteen pages is as follows: He says, "Having had occasion to treat cases of sickness in young unmarried women suffering from flexion, it has been observed by me that when those patients marry and become pregnant, the sickness observed is liable to be unusually severe and troublesome. Regarding the cases which have come to me in the course of consultation practice, and where the symptoms of sickness have been still more troublesome, the same fact holds good, the connection between the two things has been observed to exist." He relates a typical case in which anteflexion was the cause of trouble, which was relieved by placing the woman upon her back in bed, and confining her to this position, "in order to allow the expanding uterus a better chance of escaping from the pelvis, and that the bowels should be kept regularly open." He says in explanation, "the tissues of the uterus, including the nerve ramifications in it, are compressed at the seat of flexion, and this is the cause of the sickness." He recommended the use of pessaries as a means of cure.

"Dr. Barnes observed, there had been many theories advanced as to the cause of vomiting in pregnancy. Displacement of the uterus, as retroflexion and anteflexion, was an old theory. It was mentioned by Moreau; and Mayer and Ulrich give cases in which anteflexion was the cause." "He was in a position to state, from many precise observations, that flexions of the gravid uterus were often present without any unusual degree of vomiting, and that the most obstinate vomiting occurred where there was no flexion." He believed in the theory of the stretching of the uterine fibre, as set forth by Bretonnean of Tours, and brought forth in evidence the facts of vomiting resulting from intra-uterine polypus; the injection of water to induce labour; the rapid accumulation of an excess of liquor amnii: and the existence of twin pregnancy.

His theory was, that "growth that kept pace with that of the contents of the uterus did not cause vomiting; but it was caused whenever the fibre was stretched rapidly, the distending contents outrunning the accommodating growth of the uterus.

In a long-continued criticism which followed, the views of Dr. Barnes were generally concurred in, and the employment of pessaries for mechanical support condemned as unsafe.

Case of Cæsarean Section, by HENRY GIBBONS, M.R.C.S.—The subject was 3 feet 10 inches high, 22 years old, and in labour five hours. The steps of the operation were all favourable, the incision having been made prior to the rupture of the membranes, and everything bid fair for the recovery of the patient. Chloroform was employed during the operation, and in about an hour a full dose of opium was administered. "Uncontrollable vomiting commenced soon after, and continued with scarcely any intermission till she sank, the morning of the 15th January, or about forty hours after the operation." The examination of the body showed neither extravasation into the peritoneum, nor any peritonitis. The child lived nine days.

We would ask whether death resulted as a secondary effect of chloroform, or immediately, from the operation? In the reviewer's table of sixty American cases, there is no parallel instance of death. The vomiting followed too quickly the dose of opium to have resulted from it.

Case of a Child born at twenty-two weeks and two days, living for eighteen days after Birth.—The mother was 38 years old, had had nine children and three miscarriages, and the date of the conception noted appears to be well established. The fœtus was a male, and the testicles had not descended into the scrotum. Dr. C. H. F. Routh, who reports the case, mentions that of one born living at four months; another reported by Maissonneuve as respiring for six hours at four and a half months; and a third by Mr. Smyth, twelve hours, at five months. We may add another in our own experience, similar to the last, born of a woman far advanced in phthisis.

On Tetanus after Abortion, by ALFRED WILTSHIRE, M.D.—This is the title of an article covering six pages, founded upon two cases which are reported, in both of which there existed great mental depression, one arising from the fact of the conception being adulterous, and the other from cruel desertion by her husband. This is a subject of considerable interest, although of little practical value, as the disease is of great rarity in temperate climates. In hot countries where traumatic tetanus is not uncommon, puerperal tetanus is also frequently met with.

Dr. Playfair remarked that he had seen many cases in the Lying-in Hospital at Calcutta, even when there was nothing otherwise abnormal about the patient which would favour its occurrence.

On Pelvic Hæmatocele, with special Reference to its Diagnosis and Treatment, by Dr. ALFRED MEADOWS.—This article and its discussion constitute a valuable treatise upon this form of disease. There appears to be a considerable difference of opinion as to the frequency of the malady, its diagnostic features, its extremes of gravity, and the necessity or propriety of evacuating the blood through the vagina or rectum. It is impossible to make a satisfactory summary of the mass of evidence, suffice it to say, that it leaves the impression that a careful tactile and symptomatic diagnosis would reveal the existence of the disease in mild, as well as its graver forms; and experience is in favour of evacuating the fluid where it is large in quantity, not very recently effused, and is producing by its presence severe constitutional and local disturbance. There is abundant evidence to show that the peritoneal cavity is much more tolerant

of the presence of blood than of any other extraneous fluid, and rarely exhibits the signs of peritonitis as a consequence of its effusion. In retro-uterine hæmatocele the blood in time becomes encysted in some cases, and subsequently undergoes a change by which it is rendered putrescent, and a source of constitutional irritation, when it is well to liberate it by puncture.

We have seen the peritoneal cavity so largely filled with blood, that its loss from the circulation reduced the patient apparently to a moribund condition, and still there was no peritonitis resulting from its presence, or symptoms requiring its evacuation: the lady recovered her health perfectly, and several years subsequently gave birth to a healthy infant.

Dr. Robert Barnes remarks: "My own observations lead me to the opinion that the true indications for puncture are constitutional evidence of blood-poisoning from alteration in the blood-mass of the tumour; or distressing pressure upon the bladder and other pelvic viscera; and that in those cases where there is no evidence of such conditions the prospect of recovery is in no sensible degree enhanced by puncture, and that the practice, therefore, is to be shunned simply because too much diligence is bad."

Dr. Protheroe Smith recommends that the fluid, when evacuated, should be drawn off by the aspirator, and says of its use in one case: "By it every particle of matter and gas was sucked out so completely that perfect collapse of the sac took place, there was no reformation of pus, and the patient left the hospital apparently perfectly well."

On a Case of Sudden Death seventeen days after Delivery, by Dr. W. S. PLAYFAIR.—The subject was a healthy lady 30 years of age, primipara, and delivered after a short and easy labour. Convalescence progressed favourably until the tenth day, when fever, embarrassed respiration, and physical signs of pleurisy resulting from exposure to a draught, appeared. On the fifteenth day was apparently perfectly restored. On the seventeenth day ate a hearty breakfast and was anxious to get up: about noon, suddenly, whilst sitting up in bed, was attacked with cardiac pain, great dyspnœa, became livid in countenance, and was dead in about five minutes. No autopsy. Dr. Playfair remarks in explanation of the cause of death: "The slight pleuritic attack had produced an increased hyperinosis of the blood, over that already existing from the alteration due to the puerperal state. Hence the coagulation, which probably might not have occurred had it not been for the apparently trivial and temporary pleuritic mischief, which, under ordinary circumstances, would have given rise to no anxiety.

On the Diagnosis of the least known Varieties of Uterine Inflammation, by Dr. E. J. TILT.—This article, with the ensuing discussion, cannot from its nature be reproduced in abstract form. It is a paper of much value and well worthy of examination: the author condemns the effort made by Dr. Routh, to distinguish fundal endometritis from internal metritis.

On the Contractions of the Uterus throughout Pregnancy; their Physiological Effects, and their Value in the Diagnosis of Pregnancy, by Dr. J. BRAXTON HICKS.—In this well-written article the author gives the results of his examinations of the gravid and otherwise enlarged uterus, through the abdominal walls, during a period of eight years, and establishes the fact of the continued recurrence of contraction and relaxation in the muscular fibres of this organ during the last six months of pregnancy particularly, but in all cases, where it is distended to a large size by any internal growth.

Dr. Hicks remarks:—

"If the uterus be examined without friction, or any pressure beyond that

necessary for full contact of the hand, continuously, over a period of from five to twenty minutes, it will be noticed to become firm at first, and more or less flaccid, if it be firm at first. It is seldom that so long an interval occurs as that of twenty minutes; most frequently it occurs every five or ten minutes, sometimes even twice in five minutes. However in some cases I have found only one contraction in thirty minutes." . . . "The constancy with which these contractions of the uterus have always occurred to me, leaves no doubt on my mind but that it is a natural condition of pregnancy irrespective of external irritation." "In a general way the pregnant woman is not conscious of these contractions. But occasionally it happens that the uterus is more than usually sensitive, and that the contractions are accompanied by pain; and then on examination it is found that each pain she complains of is coincident with a contraction."

"Again, when the uterus has been excited by any cause, and these contractions are more than usually powerful, the woman is conscious of their presence, and by watching these we shall convince ourselves that the contractions which were before unnoticed by her, are really the same as the so-called *pains* of premature expulsion of the fœtus and also of true labour."

These remarks of Dr. Hicks remind us of a case in which this condition existed almost to the extent of expulsion. The patient was the wife of a physician, and was pregnant the second time, having miscarried with her first child, which lived but three days. When we were called to see her in July, 1864, we found that she had been suffering with recurrent painful contractions of the uterus for some days, which her husband had not succeeded in controlling. She had had no discharge per vaginam, although her symptoms closely resembled those of actual labour. When the hand was placed upon the abdomen, the contraction and relaxation were very distinctly noticeable in the walls of the uterus at each recurrence; and the expression of the woman alternately indicated pain and relief. Under the use of morphia the action of the uterus gradually diminished, until at the end of ten days she was entirely relieved. She was delivered of a living child in October. The painful contractions in this case continued altogether about three weeks.

Dr. Hicks proposes to make a practical use of this habit of the uterus, to distinguish it when enlarged from non-uterine tumours, and to detect the presence of abnormal intra-uterine growths and fluid collections by the comparative differences under palpation of the contracted and relaxed pregnant uterus; and the state under similar conditions, when distended by solid tumours, hydatids, and fluid accumulations.

Abstract of a Memoir on Osteomalacia, by Dr. Gaetano Casati, of Milan, by Dr. Robert Barnes.—Although this disease is almost unknown in the United States, it will be of interest to introduce a portion of this paper as explaining the reasons for the prevalence of the malady in some European localities, and per contra, its non-existence in any special district here.

Dr. Casati gives the experience of eight years in the Royal School of Midwifery at Milan, during which period 7719 women were delivered, of whom 62 presented evidences of clearly-marked osteomalacia.

"Almost all the women came from that part of the province of Milan known as the Valley of Olona. All the women came from wretched villages where food was scarce, consisting for the most part of maize or rice, often musty; they worked at washing or weaving; were badly lodged, their rooms being ill-ventilated, out of repair, the glass of the windows generally substituted by paper; the houses built upon clay, and damp. Added to these unhappy conditions, Casati thinks the character of the water and the vegetables with which the people eke out their scanty diet is largely concerned."

Only one of the 62 women was a primipara, and excepting five, all had had

three children, or more: 41 presented various degrees of pelvic deformity. One-half of the women were delivered normally, and of the remainder, but five required the reduction of the fœtal head, and two, the operation of Cæsarean section; 42 children survived.

It will thus be seen, that until we have a wretched, ill-fed, crowded peasantry in the United States, living in damp localities upon the ground, we are not likely to meet with osteomalacia, except in very rare instances; and that it is only in the minority of cases that destruction of the fœtus or the Cæsarean operation is required.

A Case of Extra-uterine Gestation, with Remarks on the Treatment of that Condition, by Dr. ALFRED MEADOWS.—This article presents the simple record of a case of abdominal pregnancy, in which a young woman died from rupture of the cyst and consequent internal hemorrhage, without any attempt having been made to prevent the occurrence of this accident. Dr. Meadows reflects somewhat upon himself for not having anticipated the event by the operation of gastrotomy, and proposes in such an operation to tie the fœtal cord and leave the placenta, instead of running the risks of hemorrhage by its detachment. The reading of this paper provoked a lengthly discussion upon the subjects of the diagnosis of extra-uterine pregnancy, its dangers, spontaneous cures, and the propriety and methods of surgical interference: from which it may be inferred that they thought it proper to open the abdomen in cases where rupture had already occurred, and would also recommend the anticipatory operation if the diagnosis was not so uncertain, and patients did not sometimes recover by calcification of the fœtus, or its discharge by abscess and ulceration into the vagina, rectum, or through the abdominal walls. The great obstacles to the early operation, as expressed by Drs. Hewitt and Playfair, are, the difficulties of satisfactory and certain diagnosis; the fact that cases rarely present themselves until too late for gastrotomy as an anticipatory measure; the difficulties to be met with in the form of extensive adhesions; and the dangers of uncontrollable hemorrhage. Dr. Meadows took the bolder plan, and recommended the abdominal section as soon as the diagnosis could be clearly established, regarding it as less dangerous in the average, than waiting the course of nature.

Successful Case of Ovariotomy during Pregnancy, with Remarks on the Treatment of Ovarian Tumours complicating Pregnancy, by EUGENE GODDARD, L.R.C.P.; operation by Spencer Wells.—The subject was a lady 29 years old, pregnant for the sixth time, and about two months advanced at the date of the operation. Incision five inches long; eleven and a half pints of fluid removed; weight of cyst and solid matter three and a quarter pounds. Total about fifteen pounds. Anæsthetic used, chloro-methyl. Recovery from operation rapid and complete; patient went to full maturity; labour natural; able to nurse her infant, and in as good health as after previous confinements.

In his defence of the operation during gestation, Dr. Goddard remarks:—

"Ovariotomy, now an accepted operation, has had to fight its way through more opposition and bitter prejudice than perhaps any other. Sufficient facts have, however, been accumulated to establish it now as one of the triumphs of modern surgery. But it seems that a new development is open to it. The peritoneum, it is settled, may be invaded under certain careful precautions, perhaps the most important being that no ovarian fluid be allowed to escape into its cavity; or if it do, that it be carefully removed. May we not now say the peritoneum may be safely invaded under like conditions during pregnancy? Or if it be objected that the cases are still too few to warrant such a conclusion, may we not say that the tendency of the cases on record, is to prove that ovariotomy has been thus far so good an alternative that it is only necessary in

should be followed up to become established as in many cases the *best* alternative in this complication?"

"The President remarked that in the case of a compound or solid cyst, if relief must be given, then the choice lay between induction of labour and ovariotomy. The cases of Mr. Spencer Wells showed, so far as they went, that the results of the latter had been satisfactory, and no doubt in his hands the question would receive an intelligent solution."

Fibrous Enlargement of the Uterus successfully treated by Ergot, by Dr. John Brunton.—Subject, a single woman of 47; fair complexion, and in full flesh. Uterus when first examined reached midway between pubes and umbilicus. Menses excessive, and periods about every two weeks. Treated at first by iodine to abdomen, and bromide of potassium internally, but with no benefit; then with digitalis, morphia, cannabis indica, and finally ergot. The hemorrhage becoming alarming, and the uterus reaching as high as the umbilicus, the ergot was administered in large doses, as much as from one to two ounces being given at each period, with controlling effect. The uterus began to diminish in size under this treatment, and in six or seven months could not be detected above the pelvis, and there was little or no increase in its size, examined by the rectum. She is reported to have taken from forty to fifty ounces of ergot in six months, and to have had morphia injected hypodermically very often, sometimes daily. At last report, the catamenia were still frequent and excessive, but under control by ergot and morphia; no enlargement of the uterus could be detected.

2. *Edinburgh Transactions.*—The Edinburgh Obstetrical Society was instituted in 1840, but did not publish its transactions in book form until 1870, at which time there were 109 "Ordinary," and 105 Corresponding Fellows upon its roll. The work opens with a monograph of fourteen pages entitled:—

On the Construction of the Cephalotribe, by Dr. J. Matthews Duncan, M.D.—This article is intended to point out the errors in the construction of the short handled, or Edinburgh instrument (Sir James Y. Simpson's); and institute a comparison between its efficiency and that of the Paris instrument (a modification of Baudelocque's). Dr. Duncan made a series of experiments upon the relative powers of the two instruments, and condemns the short handled variety as deficient in leverage; not shutting the blades closely enough before the handles are brought in contact; and having too much spring in the former, without there being any compensation in the set of the handles, which should pass over each other. He recommends a slight modification of the Paris instrument, giving it a strong button-pivot, thin but broad handles, and a free passage of the latter over each other, so as to allow the screw to be turned until the handle ends are in close contact, thus reducing the weight from 4 lbs. 1 oz. to 3 lbs. 5 oz., without diminishing the necessary strength of the cephalotribe.

The reading of this paper occasioned a considerable discussion, parties admitting the value of many of the views advanced by Dr. Duncan, but at the same time claiming an efficiency in the Edinburgh instrument far beyond what he had apportioned it: Dr. Keiller stating that he had repeatedly used the condemned cephalotribe, and had had no great difficulty in crushing the fœtal head with it. The general impression appears to have been, that the instrument of Dr. Duncan was still unnecessarily cumbersome.

Notes of a Case of Puerperal Mania, by Dr. Menzies.—This is chiefly valuable as an instance of the effect of bromide of potassium, the patient having taken four ounces during six weeks, with an intermission of eight days. The

notes of the case from day to day do not show any very satisfactory effects from the remedy. Dr. Wilson thought that the recovery of the patient had not been more rapid than many cases of mania treated without the bromide.

Dr. Ritchie mentioned a case in which the bromide failed, the patient became much reduced, would take no food, and ultimately died. He held the opinion that puerperal mania was in most instances the result of nervous exhaustion and required a stimulating treatment.

Birth of a Child weighing Seventeen Pounds, by PETER BROTHERSTON, F.R.C.S.E.—The mother was five feet high, aged 40, and in labour with her seventh child. The head came away normally, but the body could not be delivered by manual assistance, and the child perished before it was finally removed by the blunt-hook. Length of fœtus, 23½ inches; occipito-frontal circumference, 15 inches; circumference of shoulders, 20; chest, 15½; abdomen, 17; funis to head, 13; to sole of foot, 10½. Length of arm, 10 inches. The child was perfectly formed in every respect. The father was 44, six feet high, and weighed 182 pounds. So much force was required in delivering the body of the child, that its right humerus gave way near the neck under the pressure of the blunt-hook.

How long may a Dead Fœtus remain in the Uterus?—This question is propounded by Dr. JAMES YOUNG, who relates the case of a woman æt. 36, mother of four children, who, becoming insane, was sent to an asylum, where she in a measure recovered, returned home, conceived, was recommitted for three months, after which she became well, and so continued. She became pregnant during six days when at home in January, and was not again home until April, felt the motions of the fœtus until the end of June, after which there was no more motion or increase of size, and breasts became small and flaccid. Fœtus expelled in September, putrid, and inclosed in the membranes; measured when extended, ten inches. Dr. Young computes the age of the fœtus at its death at five calendar months and ten days, and that the mother continued to carry it two months and ten days longer, without any detriment to her health.

Mr. PRIDIE reported a case in which he made the calculation that more than three months had expired between the death of the fœtus and its expulsion. The fœtus died from the effects of fear, excitement, and over-exertion on the part of the mother in escaping from a party of cavalry on drill.

Transverse Contraction of the Inferior Straight; Caries of Ischium, with Sequestrum projecting into Vagina; Cephalotripsy, and Recovery, by Dr. PAUL GRENSER, of Dresden.—Woman unmarried, æt. 20; four feet three inches in height. Tubera ischii closely approximated, scarcely admitting two fingers; one inch above the vulva a splinter of bone, about an inch and a quarter long, was found projecting into the vagina, and arising from the ascending ramus of the right ischium. This bone was removed without hemorrhage; labour continued sixty-eight hours before full dilatation of os uteri was effected. Forceps then applied, which occasioned considerable hemorrhage, supposed to come from the carious ischium; head drawn down, perforated, crushed with cephalotribe of Busch, and child delivered. Woman left the hospital on the ninth day. This carious condition of the pelvis had existed from the age of seven, at which time of life she had vaginitis, purulent discharge, and subsequently escape of fragments of dead bone. For nine years prior to her labour, the discharge had been slight. The inferior straight measured 3 inches 4 lines antero-posteriorly, and 2 inches 2 lines between the tubera ischii.

Caoutchouc Dilator.—Dr. KEILLER remarked: "The mode of inducing premature labour by means of caoutchouc bags was first brought before this Society (as reference to its proceedings will prove), and several Fellows now

present had been made practically conversant with this plan before it was adopted by the gentleman whose name it now generally bears."

Sir James Simpson and other members replied, that if any name ought to be attached to this special mode of practice, it was unquestionably that of Dr. Keiller, and not of Dr. Barnes. It was only fair to Dr. Keiller and to the Society that this should be known and acknowledged.

Tetrachloride of Carbon.—Two cases of labour reported by Dr. Inglis, in which this anæsthetic was used. First patient took it undiluted, resulting in violent uterine action in first stage of labour, and syncope under its full impression. In second case (one of uterine inertia), given with an equal portion of chloroform, resulted in bringing on effective expulsive action at once.

Sir James Simpson found that it depressed the pulse, but did not find it produced syncope; had used nitrite of amyl, but it did not succeed in labour, although a pain-reliever in dysmenorrhœa, cancer, headache, etc.

Spontaneous Healing of Harelip in Utero. Case reported by Dr. Cuthbert. —Scar well marked on both sides of lip, soft palate fissured. Child being at age of three months when reported.

Complete Separation of the Placenta before Labour without external loss of Blood.—Dr. James Cappie reports two cases where apparently the whole placenta had become suddenly detached from the inner surface of the uterus. The first accident happened as the woman was getting out of bed in the morning, in the seventh month of her pregnancy, having been aroused by severe abdominal pain. She was delivered as soon as practicable with the forceps. The placenta was remarkably soft, and the child affected with general anasarca. The mother was pale and delicate, but had no albuminuria. The second patient was in excellent health, and like the other in her fifth pregnancy, but advanced about seven and a half months, was seized suddenly, without assignable cause, with abdominal pain followed by giddiness and fainting, and upon recovering consciousness experienced a sensation of increased distension. Os uteri found closed; no evidence of labour; forcibly introduced the finger and commenced dilatation, but without separating the membranes to any extent; labour excited, advanced rapidly; child delivered in a flaccid, anæmic state in less than two hours. Placenta presented a tear in its centre, but was otherwise healthy. Both women recovered. The reasons given for believing in the entire separation of the placentæ were, that in both cases they were extracted before any clots were discharged; were found low in the vagina, and the membranes inverted by the gush of discharge, carrying the placentæ in advance. The external character of the uterus under palpation, in both instances, appears to have given evidence in favour of entire separation, a very rare condition as compared with the number of cases which are only partial.

Cephalotripsy after Turning, by Dr. Keiller.—In this case the woman was in her second labour, the first having been terminated by instruments after several days' suffering. Dr. Pridie was engaged to attend her with the second, and called in the aid of Dr. K. Turning was resorted to in preference to craniotomy, in the hope of possibly saving the child, but when the body was extracted the impossibility of delivering the head entire was discovered; it was then perforated, the body slowly drawn upon, and pressure made upon the head from above the pubis, when at the maximum of effort the body separated at the neck. Forceps, crotchet, craniotomy forceps, and cranioclast were tried in succession, but failed. The cephalotribe of Simpson was then obtained, and the head crushed and removed. The woman made a good recovery. Dr. Keiller advocates the "Edinburgh Cephalotribe," as that of Prof. Simpson is generally called, in preference to the longer and heavier modifications of Baudelocque.

On the Scarification of the Gums of Infants in Dentition, by Dr. CAIRNS.—This article is an effort to prove that the operation does not relieve pain, remove tension, prevent or check convulsions and diarrhœa, and is a preventive of the subsequent ready protrusion of the tooth by the formation of a dense cicatrix. In the discussion which ensued the current of opinion was decidedly against him, the only concession made being that the operation was performed oftener than there was any occasion for.

On the Combined External and Internal Method of Version, with cases, by Dr. ALEXANDER MILNE.—This article is written in advocacy of the method of delivery by the feet, instead of the use of the forceps, Dr. M. believing from his experience of forty cases of version by the old method, that under chloroform, the operation is attendant with very little danger. He recommends in cases where the os is rigid, or but slightly dilated, and especially where the membranes are intact, that the bi-manual method of Braxton Hicks should be employed. He reports four cases in which he used the combined method. The first, Presentation of head and funis—version readily accomplished before rupture of membranes. Second, Moderately contracted pelvis, woman delivered once by forceps, and twice by turning; os small and rigid, version as before, but more difficult, as the amniotic fluid was believed scanty, and the abdominal walls were greater in thickness. Third, Placenta prævia with profuse flooding —version accomplished mainly by external pressure, woman and child saved. Fourth, Convulsions, no labour-pains, used forcible dilatation with fingers, version before rupture of membranes, both saved.

On the Use of the Chlorate of Potash in Certain Cases of Pregnancy, by Dr. CUTHBERT.—Case reported where five abortions had occurred in six years after marriage, in all of which the placentæ were diseased except one, in which the labour was induced by a fall at the sixth month. In a sixth pregnancy was kept much of the time in a recumbent position, and under the effects of the chlorate of potash: gave birth to a healthy child.

In the discussion which ensued, much credit was given to the chlorate, in similar cases. Sir James Y. Simpson had used it in many instances, and recommended it in doses of ten to twenty grains three times a day, combined with rest.

Consecutive Malformation.—As an instance of this, Dr. Menzies reports three successive births of children affected with spina bifida, the offspring of the same mother. The reviewer has a patient whose first and second children are hypospadiacs, and whose only sister has a son similarly malformed. It is not hereditary in either instance.

Ovarian Cyst ruptured by Accident, resulting in Cure, by THOMAS L. M'MILLAN, L.R.C.S.E.—The subject was single, aged 42, labouring under chronic bronchitis, emaciated and somewhat cyanotic, case deemed very unfavourable: was knocked down and injured by a runaway horse attached to a cart; taken up insensible, and remained unconscious for several days; upon recovering her senses, found abdominal tumour had disappeared. After being confined to bed six weeks, and suffering with a severe cough, she entirely regained her health in every particular, and was permanently cured of her dropsy.

Two Cases of Convulsions during Dentition arrested by Scarification of the Gums, by G. STEVENSON SMITH, L.R.C.S.E.—This is an answer to the paper by Dr. Cairns, previously quoted, taking an entirely opposite view of the value of scarification, and giving cases in evidence of its power to arrest convulsions, relieve pain, etc. His opinions were sustained by the views and experience of the several Fellows who took part in the discussion of the question.

R. P. H.

ART. XXI.—*Transactions of American State Medical Societies.*

1. *Transactions of the Iowa State Medical Society; from May*, 1867, *to April*, 1871, *both dates inclusive.* 8vo. pp. 272. Davenport, 1871.
2. *Transactions of the Medical Society of the State of California, during the Years* 1870 *and* 1871. 8vo. pp. 252. Sacramento, 1872.
3. *Transactions of the State Medical Society of Michigan, at its Fifth Annual Session, June*, 1871. 8vo. pp. 64. Lansing, 1871.

1. THE volume before us is the first of the *Transactions of the Iowa State Medical Society* that have appeared. It comprises, in addition to an abstract of its proceedings at its several annual sessions from May, 1867, to April, 1871, both inclusive, a selection of the papers and reports read before the Society at these sessions respectively.

The opening Address of the President, J. W. H. BAKER, M.D., at the session of 1867, presents a brief notice of the change, which of late years has taken place in the treatment of acute diseases; of the prevalence of criminal abortion; of the subject of female practitioners of medicine; of the evil resulting from newspaper advertisements recommendatory of patent medicines, nostrums, and specifics for the cure of every ailment incident to the human frame.

The Presidential Address, delivered at the session of 1868, by WM. WATSON, M.D., is mainly taken up with a notice of the past history and progress of the Iowa State Medical Society; the difficulties, the results that have been already accomplished by its agency, and its present prospects for the future.

The Address of the President, P. HARVEY, M.D., at the session of 1869, presents a well-drawn up, and interesting sketch of the "Climatology of Iowa," more especially of its southern portion.

The Presidential Address, delivered by S. B. THRALL, M.D., at the annual session of 1870, has reference mainly to events connected with the medical profession in Iowa.

The first paper read at the session of 1867, was an account of a "Misplaced Ovary," by Dr. E. WHINERY. May, 1867, an unmarried female, 26 years old, consulted the doctor in regard to an apparent cyst or tumour, in her left labium. When she first noticed it, it was about the size of a large bean. About six months afterwards it began to grow, and recently had increased in size up to that of a walnut. It was firm to the touch, and interfered somewhat with the act of walking; it could be moved backwards toward the posterior commissure. Extirpation with the knife was advised, and after a short delay submitted to. Dr. W., with a sweep of the scalpel, in the line of the labium, laid bare the tumour, which had a tunic similar to the *tunica albuginea testis*, and an envelopment corresponding with the *tunica vaginalis testis*. These being incised, a misplaced ovary was turned out, of a kidney shape, with a pedicle proceeding from its pelvis. The pedicle was ligated and divided. The length of the ovary thus detached was two inches, its width an inch and a half, its thickness one inch. The wound healed mostly by the first intention. The ligature came away in six days, leaving a small fistulous opening, from which a semi-purulent matter was, for some six weeks, occasionally discharged. It is uncertain at what period of life the ovary had descended through the inguinal canal, but it was probably during early infancy. The patient stated, a year after the operation, that her catamenial discharge amounted then to only one-half the quantity it did formerly. She remains a fine healthy woman, still unmarried.

Next is the report of a rather rare case, in which four successful operations for the removal of stone from the bladder were performed by the bilateral method at comparatively short intervals. The report is made by Professor J. O. HUGHES, M.D.

Dr. E. WHINERY relates what he terms "A Novel Case of Lithotomy." In the latter part of November, 1866, a man, supposed to be labouring under stone in the bladder, was operated on by the lateral method, by Dr. WHARER. The incision was made carefully but boldly, in view of the supposed large size of the calculus. The entrance of the knife into the bladder was not followed, as was expected, by a free gush of urine, nor until search was made for the stone. Upon introducing the finger, it was ascertained that there was but apparently a calculus. Some ineffectual efforts were made to seize it. Dr. Whinery now made an examination. The index finger of his left hand met the foreign body as it was apparently being forced downwards and by the forceps passed along the finger, the body was seized and withdrawn. It proved to be a common lead-pencil, nearly four inches long, and partially sharpened at one end, its nearly entire surface being encrusted with a calculous deposit, varying in thickness from a mere shell to two inches in thickness, and quite rough. The patient recovered. He stated that he supposed the pencil may have been introduced by the inmates of a house of ill-fame, whilst he was there in a state of thorough intoxication. This he inferred simply from the fact that his trouble in the bladder came on soon after his visit, whilst drunk, to that house.

The views advanced by Dr. J. C. STONE, in his report on the subject of "Criminal Abortion" are perfectly sound, and will receive the hearty indorsement of every legitimate member of our profession. The suppression of the horrible and growing crime of fœticide must be left to stringent enactments against it by our legislatures, and to the prompt and unflinching action of our public officers, and our criminal courts and juries in the carrying those enactments into force.

Dr. J. C. HUGHES describes a new operation, by which "Distortion of the Long Bones" may be remedied without reduction of their length. It is in some degree a modification of Dr. Barton's well-known operation for straightening distortion of the femur.

The first communication at the session of 1868, by Dr. WM. GULCH, on "Obstetrical Statistics," is interesting and instructive so far as it goes; but presents no new facts.

A curious case of "United Twins" was described by Dr. P. Harvey. The labour was of ordinary duration. In all other respects, save their abnormal union, the children were well developed, and of the full ordinary size of twins. Weight, nearly eleven pounds. They were united from neck to umbilicus. The ribs and clavicles of each were continued to a common sternum, being so arranged as to form one common large thoracic cavity. There was but one funis containing four arteries, but only one vein; it was inserted at the centre of the lower part of the junction. The livers were united, with two gall-bladders. The other abdominal viscera were distinct and normal. In the common thorax were contained two sets of lungs, smaller than usual, weighing only 1½ oz. There was a large double thymus gland. Only one heart, weighing about one ounce, flattened, and suspended diagonally across the centre of the thorax. Instead of being a combination of two hearts, it was perfectly unique in its anatomy, and capable of subserving the purposes of intra-uterine life only. It had but a single auricle, into which opened two systemic and two pulmonary veins. The auricle communicated by two openings, each guarded by a pair of mitral valves, with a large muscular ventricle, from which issued two aortæ of the ordi-

nary size, with perfect semilunar valves. On each side of this large ventricle was a smaller one, communicating freely with it. A small pulmonary artery, destitute of valves, and not more than one-tenth the size of the aortæ, passed from each of the small ventricles. From this description of the circulatory organs of these united twins, it must be evident that only a small quantity of blood could have reached the lungs, much too small for the respiratory purposes of a high organization. Cold-blooded life alone could not be sustained with such a heart; hence these twins were not viable.

Dr. E. WHINERY related a most interesting case of "Rupture of the Uterus," occurring during labour, in a healthy female about 37 years of age, in which the child was removed by the abdominal section. At the end of six days from the time of operation the wound in the abdominal parietes had healed by first intention, and the patient sat up in bed for three or four hours. Five days later the lochia had ceased, and the patient went about her ordinary housework. On the 1st of June she menstruated, and again on July 1. She then became pregnant, and on the first day of April, 1866, about twelve months subsequent to the operation, she gave birth to a healthy female child, her labour during its latter stage being aided by the use of the forceps. There was nothing peculiar attendant upon gestation. It was, she said, her tenth pregnancy, and the easiest delivery she had ever had. Two of her children had been stillborn, in consequence of protracted and difficult labour.

The first paper reported to have been read at the annual session of 1869 is a very short and unsatisfactory one on the "Uses and Abuses of Mercury," by Dr. F. LLOYD.

An instructive case of "Gunshot Wound of the Abdomen" from a Minié-ball was reported by Dr. J. F. KENNEDY. The ball entered near the umbilicus, and, passing entirely through the abdomen, emerged a little below the right kidney, near the spine. When examined by Dr. K., three or four days after the receipt of the wound, he found that there was a free discharge of fecal matter. The patient was placed on his back, and the wound treated by the simple cold water dressing. The bowels were, as far as possible, kept quiet by opium, while the rectum was occasionally washed out by simple enemata. In about ten days the anterior wound healed up kindly. As to the lower wound, now about an inch in diameter, it was discovered that the intestine had become adherent to the parietes of the abdomen. Paring the edges of the opening, they were approximated, and retained in position by adhesive strips. Free discharges of fecal matter took place per anum, and the patient recovered rapidly. In a month he was discharged, and sent to his home in Chicago.

The same gentleman gave the history of a case of "Fracture of the Skull," with loss of brain-substance, terminating in recovery.

At the annual session of 1870, a very sensible paper on "Orthopædic Surgery" was read by Dr. WM. WATSON.

A paper was also read by Dr. H. T. CLEAVER on "Diseases of the Uterus," comprising the history of two cases. The first was a case of retroflexion, with cervical endometritis, dysmenorrhœa, menorrhagia, leucorrhœa, and sterility of over ten years' duration, relieved by reposition of the organ—sustaining it *in situ* by Dr. Hodge's lever pessary, and daily vaginal irrigation by an infusion of hops, with tonics internally.

The second case was one of retroversion, with cervical hypertrophy, dysmenorrhœa, and leucorrhœa of about four months' duration, treated and relieved in the same manner, without resort to either the caustic or alterative means generally recommended in uterine affections of a similar character.

The views advanced by Dr. J. G. HOUSE, in his "Report on Venesection,"

we believe to be sound. There is evidently beginning to be felt a reaction in regard to this measure—at least in the opinions and practice of some authoritative teachers and practitioners in Europe. There is little likelihood, however, that, in future, the practice of venesection will be carried to the same excess as it was formerly. Our improved pathological knowledge in respect to many diseases has greatly increased during the last twenty-five years, and cannot fail to cause a more discriminating and careful resort to so potent an agent as bloodletting. Our own experience of over half a century, in common with that of Dr. H., leads us to believe that, in some conditions of the system, detraction of blood is decidedly useful, in certain cases, as, for instance, of puerperal convulsions, in order to protect the brain from violent and dangerous congestion, previously to the administration of chloroform, as well as in other great congestions of the brain and lungs. In pneumonia and pleurisy, also, especially when complicated by advanced pregnancy, in traumatic fever, where the appetite and power of assimilation are unimpaired, leading to excessive plethora, and endangering the patient by the occurrence of apoplexy of the brain or heart.

In a short paper by Dr. E. H. HAZEN the true doctrine of "Revaccination" is very clearly laid down. Revaccination is shown to be the useful and indispensable complement of a first vaccination—not that it is always successful, but in order that certainty may be arrived at that all disposition to the receptivity of variola has been permanently extinguished in the individual.

Dr. J. W. H. BAKER related a case of "Death resulting from the Use of a so-called 'Hair Restorer,'" the main ingredient of which was shown to be acetate of lead. Death was preceded for some time by well-marked symptoms of lead-poisoning.

The same gentleman read the history of a case of "Purulent Pleuritis." The pus effused into the thoracic cavity was discharged by puncturing the walls of the thorax. The pus that flowed out, after a gush of air, was very consistent and fetid. After about three pounds were removed, the opening was dressed by a simple compress. For the next ten or twelve days, each day, from a half to one or two pounds of pus were allowed to escape. Finally a medium-sized elastic catheter was introduced into the thorax, and passed some six inches into the fluid contents, while to the free end some rubber tubing was attached, and to this, temporarily, a rubber suction ball, by which the catheter and its tubing being freed from any existing impediment, the purulent matter would flow out in a continuous stream. In this manner over thirty more pounds were removed from the thoracic cavity. The general treatment of the patient was by tonics and other remedies adapted to his varying symptoms. A great difficulty arose from the clogging of the inner lateral orifice of the catheter by a mass of floating shreds of fibrous tissue. To get rid of these the catheter had to be withdrawn, and the fibrous mass carefully removed by a spring forceps. On one occasion the mass amounted to about eight ounces. The discharge from the thorax becoming less in quantity and of improved quality, injections were made into the cavity of a solution of carbolic acid. At first, one pound at a time, of the strength of ten grains acid to one ounce water. The injections, gradually increased in strength, were repeated once a week. As the discharge lessened the respiration gradually improved. The effusion of matter into the cavity of the chest, at the end of five months and ten days, entirely ceased, and the health and nutrition of the patient were in a short time fully re-established.

At the annual session of 1872, Dr. F. LLOYD read a paper on the subject of our "Code of Medical Ethics."

Dr. J. R. GARRELL related the history of a case simulating somewhat fracture

of the femur; its progress and termination proved, however, that it was one of periostitis, causing destruction of bone.

The volume closes with a series of biographical notices of deceased members of the Society, a list of its present members, and of those who have ceased to be members.

2. The volume of *Transactions of the Medical Society of the State of California*, issued in 1872, comprises an account of the proceedings eventuating in the reorganization of the Society, as well as the Transactions of the first regular annual session of 1871.

This was inaugurated by a very able address from the president, THOMAS M. LOGAN, M.D., on "The past, the present, and the future Interests of the California State Medical Society."

Dr. L. C. LANE read a paper on "Coxalgia, and its Treatment," which presents an admirable description of the disease, with remarks as to its different modes of treatment.

A tolerably full account of the leading features comprising the "Medical Topography of St. Diego County, Cal.," was presented by Dr. D. B. HOFFMAN, and a short notice of that of Santa Barbara County, by Dr. M. H. BIGGS. In respect to St. Diego, Dr. H. remarks: "The primary diseases of this locality are bilious and remittent fevers and rheumatism." In the second report Dr. Biggs points out as the most remarkable feature of Santa Barbara its immunity from all epidemics. There are no malarious diseases—persons who come to Santa Barbara labouring under fever and ague, soon become rid of it. During a residence of twenty years, Dr. Biggs has only seen one case of membranous croup, and has heard of but two others.

"The Medical Topography of Petaluma" is briefly sketched by Dr. G. W. GRAVES. As to the diseases of this county, neither intermittent nor remittent fevers are ever met with in the vicinity, except in such persons as had been recently exposed to malarious influences elsewhere. Typhoid fever is very rare, and pneumonia is almost unknown. There are few endemic diseases, and no epidemic has been troublesome for a number of years except the smallpox, in the winter of 1868–9. Consumption and chronic bronchitis are, however, of rather common occurrence. The climate also is unfavourable to their cure or amelioration. With the exception of the two latter diseases, the county is one of the healthiest in the State.

Dr. R. W. MURPHY describes an ingenious splint, accompanied with a drawing, for the treatment of "Fracture of the inferior maxillary bone."

An able and instructive paper, illustrated by numerous well-devised diagrams, was read by Dr. E. M. CURTIS, in reply to the question, "Why do we wear spectacles?"

The report, by Dr. CALEB CUSHING, on certain resolutions that had been offered in disapproval of the dispensing of patent medicines or nostrums, and their recommendation by physicians, concludes that the adoption of said resolutions would be a work of supererogation, inasmuch as the *Code of Medical Ethics* approved by this Society, "so completely covers the same ground as that occupied by the resolutions referred to the committee."

Dr. A. B. STOUT read an able report on "Hygiene of Cities," having especial reference to the sewerage of the city of San Francisco.

From the very able report of Dr. HENRY GIBBONS, Jr., on "Practical Medicine," we can only make room for the following interesting extract:—

"The climate of San Francisco is generally thought to be unfavourable to consumptives. But a comparison with the interior, in regard to the propor-

tion of deaths from pulmonary phthisis, will not sustain that view. Of the 3112 deaths in the city, 515 were reported from phthisis; of 1666 deaths elsewhere, 264 were from phthisis, making within a trifle of one-sixth in both cases, the balance being slightly against the city. A few consumptive patients are sent from the city to die in the interior, but a much larger number are sent in the opposite direction, and end their lives in the hospitals of the city." In view of these facts and figures, it is fair to conclude that the climate of San Francisco is as favourable to consumptives, to say the least, as that of the interior towns. "The equable climate of the southern section of the State, its exemption from extremes of cold in winter or of heat in summer, its dry and pleasant winters, would seem to sustain the claim set up for it as a home for consumptives. But the figures do not confirm this. The number of deaths attributed to phthisis in Los Angelos, San Diego, and San Luis Obispo is 50 out of 197, or a trifle over one-fourth. Los Angelos makes the best show, phthisis being charged with one death in five, while San Diego and San Luis Obispo have one in three. From Santa Barbara we have returns for only four months, which give one of three deaths from consumption. But we must not attach too much importance to these partial returns. To be conclusive, statistics must cover a longer period of time, and cases coming from abroad must be excluded. Again, the number of deaths by consumption should be compared with the entire population, and not with the whole number of deaths from all causes; otherwise a small aggregate death-rate would give an undue prominence to the mortality from phthisis."

3. The fifth annual session of the *State Medical Society of Michigan*, June, 1871, was opened by a neat and appropriate address from the President, D. J. BARTHOLOMEW.

The first report is from the committee on the "Treatment of the Insane of the State, etc.," through its chairman, J. H. JEROME, M.D. The most interesting section of the report of the Committee on the "Mineral Springs and Wells of Michigan," is that drawn up by Dr. KEDZIE, on the magnetic condition of these wells.

In respect to the curative virtues of the several springs and wells nothing very reliable is known. In the cases of paralysis reported, there seems to be some good reason for believing that the water of these springs will prove beneficial, as also in those of neuralgia. But in the vast majority of cases, after deducting the beneficial influence of change of air, diet, habits, and associations upon invalids, there appears to the committee to be very little evidence that the waters of these springs are endowed with very special healing qualities.

The report of the Committee on "Vital Statistics," which closes the volume, is occupied with an exposition of the value of such statistics as the basis of prophylactic medicine, and of the mode of collecting the proper material for their construction. The report was presented by Dr. H. B. BAKER, Chairman of the Committee.

D. F. C.

ART. XXII.—*First Annual Report of the Board of Health of the Health Department of the City of New York, April* 11, 1870, to *April* 10, 1871. 8vo. pp. 628.

So much good has been accomplished, both in sanitary reform and in scientific inquiry, by the labours of the Metropolitan Board of Health of New York, that a change in its organization may be viewed with some anxiety. It is a satisfaction to learn, however, that, in the separation of the administrative Health Department of the City from that of the "Metropolitan Sanitary Dis-

trict," comprising several counties, nothing appears to have been lost of the authority conferred by recent legislation; while method and energy still characterize the efforts of the Board. Bureaus of "Sanitary Inspection," and of "Vital Statistics" have been created; a revised sanitary code has been completed; and we find in this volume a number of elaborate reports, giving evidence of much activity, and presenting facts of great value to all interested (as who should not be?) in the progress of sanitary science. We have noticed, in former numbers of the Journal, the reports of the two previous years; there is thus less of a general kind to be remarked upon now; but some of the comparative statements are important, and new subjects, with enlarged investigations of older ones, demand attention.

After the official summary by the presiding officer of the Board, the sanitary superintendent, Dr. Moreau Morris, presents the first extended report. Opening with a reference to the duties of his bureau, Dr. Morris states that *experts* are now selected by the Board for special services under its direction; educated physicians and sanitarians to inquire into causes of unhealthfulness, and skilful chemists, engineers, and mechanics for other departments. One of the most gratifying results of their continued experience has been, that, in marked contrast with former years, the health inspectors are now received in a friendly spirit by those amongst whom their duties lie, in the tenement houses and elsewhere. Increase of health and comfort seem to be growingly appreciated, even by those who have always much neglected them. One of the greatest services, perhaps, rendered by such a Board, is the *education of the public* in the application of common sense and knowledge to self-preservation and wholesome living. It has, undoubtedly, been an indispensable part of this instruction, that the Board has been enabled to *enforce* its precepts, when needful; "owners and agents having learned from experience that a prompt compliance with the orders of the Board, or even with the verbal suggestions of the inspectors, is far more profitable than any attempt at evasion or resistance through the medium of the courts." They have learned, also, that "attention to the sanitary condition of their property begets in their tenants a corresponding degree of attention and care in the use of the premises; and is, moreover, a matter of pecuniary interest, as tending directly to secure good tenants and prompt payments."

In connection with this subject, there is much interest in a communication from Dr. H. I. Bowditch of Boston, cited in this volume, concerning the success and usefulness of the London "Improved Industrial Dwelling Company." By the efforts of a liberal capitalist, Sir Sydney Waterlow, and Mr. Allen, a practical mason, over four hundred buildings have been erected for working men, combining salubrity and comfort with economy. The result has been, not only immense advantage to those for whom they were constructed, but six per cent. (or more) of annual profit to the owners. The example of such success, moreover, tends to elevate the whole character of a neighbourhood.

Of course, perfection is not to be expected. Evasion of the law has yet been practicable in New York; so that, in one tenement house, at least, but 1288 cubic feet of air are afforded to the four or six persons occupying each of its bed-rooms and living-rooms. "Gotham Court," as described by Inspector H. G. Stiles, M.D., remains to exemplify the miserable "packing-box" system in some of its worst features.

"Suffice it to say, that the two 'barrack' buildings on the court furnish tenements (two rooms each) for one hundred and forty-six families, or (averaging four persons only to a family) five hundred and eighty-four individuals, and has, at times, been packed with nearly double that number. The *roof* is a

general playground for children, and *a place of deposit* for *ashes, garbage*, and to a large extent *is used as a privy* by the tenants, whom I cannot blame for preferring it to the foul, dark, and dangerous underground privies, of which more anon." . . "The *cellars* are *dark, horribly foul, and filled with mud, rubbish and human excrement.*" . . "The *privies* (which open into and are reached through the cellars) are horrible breeding tanks of disease."

"*Ventilation* of the rooms (that is, *thorough* ventilation) is *impossible* in the present state of things." . . "As a result of these conditions, I found, at the time of my visits, *thirty-six tenements unoccupied*, most of which were on the ground-floor of the main court, proving that these people, poor as they are, cannot occupy these lower rooms. . . "Gotham Court is a nuisance which, from its very magnitude, is assumed to be unremovable and irremediable. I venture to suggest that it is neither." . . . .

From such an account we turn with pleasure to the statement by Dr. C. P. Russel, in his report upon Vital Statistics, of what has actually been effected in the conflict with these enormous evils. In 1868, the tenement houses and charitable institutions (mainly supplied from the former) furnished together 75.79 of the total mortality. In 1869 they gave 68.94 per cent.; in 1870 but 66.22 per cent. of the whole mortality of the city. Here is a gain of 9½ per cent. within three years; amounting to the saving of 2600 lives, due to the sanitary improvement of houses and of their surrounding local conditions.

Upon examining the record of births, marriages, and deaths, we find some figures, not remarked upon at length in the report, which deserve a moment's attention.

The number of *deaths* for the year among natives of this country is set down as 17,461; of those of foreign birth, 9714. Natives (of both sexes) *married* during the year, 5170; foreigners, 10,319. *Births*, of native parents, 2553; both parents foreign, 9282. We do not meet with any statement of the proportion of native to foreign population in the city; but it is a noticeable fact, that while the number of deaths of natives is not very far short of being double that of foreigners, the number of marriages is about twice as large among the latter. If this be, possibly, accounted for by the majority of emigrants being young, marriageable persons, it remains to be more striking, and less easily explained, that the number of *births* of children both of whose parents are foreign is *almost four to one* of those whose parents are both native. With a certain feeling of reluctance, we cannot help seeing in these results some degree of confirmation of the much-controverted views of Dr. Nathan Allen in regard to the decline of productiveness in our American population. As with other subjects of vital statistics, however, the complexity of the problem they present enjoins great caution in drawing deductions.

The total number of deaths in New York for the year was 27,105; being in the proportion of 28.8 to 1000 of the estimated population. From the very full and well-arranged charts, we learn, amongst other points, that by far the greatest mortality occurred in July, during the hottest weather; the least, in June, November, and the latter part of January. It is interesting, in this connection, to glance across the continent, at the record for two years, of the (24) principal cities and towns of California;[1] where we find the largest number of deaths to have occurred in November and January, and the smallest, in April, May, and June. In New York, as in other years, cholera infantum and other "diarrhœal affections" have caused the mid-summer excess; 95 per cent. of the number of such deaths for the summer quarter being in children less than five years old.

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[1] First Biennial Report of the State Board of Health of California, for the years 1870 and 1871.

From the direct effects of solar heat an unusual number of deaths took place in the same quarter; 238 in all. By the chart it appears that, while the greatest fatality from this cause (39 deaths) accompanied the highest daily temperature (July 18th), the next greatest (34 deaths) was upon a day with a maximum heat 8° lower; and this latter temperature was reached or exceeded on eight subsequent days in July and August, with only one death from solar heat on either. Some other cause besides heat alone must be here concerned. Dr. Russel alludes, without accepting it, to the hypothesis of Dr. G. Thin of Shanghae, that heat-stroke is connected in some way with electrical disturbances of the atmosphere.

Smallpox and varioloid are credited with 293 deaths for the year. This is so much less than the mortality of Philadelphia from the same cause for 1871, that it cannot be regarded as portentous. It gives occasion, however, for the following remarks from J. S. Bosworth, president of the Board, in his report:—

"The extraordinary prevalence of smallpox over various parts of the globe, especially in countries where vaccination has long been efficiently practised; its occurrence in its most fatal form in persons who gave evidence of having been well vaccinated, and the remarkable susceptibility of people of all ages to successful vaccination, are new facts in the history of this pestilence which must lead to a reinvestigation of the whole subject of vaccination, and of its claims as a protecting agent. There can be no doubt that there is a growing scepticism in the public mind as to the real value of vaccination, and this scepticism has been much strengthened by the repeated occurrence of smallpox, during the past year, in persons who were presumed to have been properly vaccinated. But the medical profession, and especially the more advanced students of contagious diseases, hold firmly to the faith of Jenner, that vaccination is a sure preventive of smallpox when the individual is brought completely under its influence."

Our confidence remains assured in the doctrine expressed in these last words; but, undoubtedly, recent experience has placed emphasis upon the requisite *conditions* for the protective agency of vaccination; it affords security only "*when the individual is brought completely under its influence.*" The era of *careless* vaccination, as mere routine, must end; revaccination (at least once) must be made, not optional, but universal; and as much care, with repetition upon failure, must be used with the second as with the first operation. We cannot quite accord with Dr. Russel in his apprehension of there being evidence of the action of some "occult laws" determining the epidemic prevalence of smallpox, in the fact that, in Scotland, in 1865, two years after the enactment of a compulsory vaccination law, variola was more prevalent and fatal than in 1858, before its passage. The time, we conceive, had not then sufficed for the full enforcement of the law to take effect. Three years later it was recorded that smallpox had almost disappeared from Scotland, as well as from Ireland. The Sanitary Committee of the New York Board, consisting of Drs. G. Ceccarini, M. Gross, and S. Smith, urges the importance, in addition to thorough vaccination, of *isolation of the sick*, and *local disinfection*, against smallpox, as well as against other contagious diseases. It is insisted, also, that special hospitals for such maladies should be placed under the immediate control of the Board of Health. The compulsory removal to hospitals of patients affected with contagious disorders has sometimes been deemed necessary.

A report upon *Disinfection*, embodying the recent experience of the officers and agents of the Board, is presented by H. Endemann, Ph.D., Assistant Chemist. It pronounces favourably in regard to the value of lime, as the cheapest and most effectual disinfectant for gutters; and states the failure, upon trial, of the "dead," or heavy oil of coal tar now in market, asserted to be

deprived of its carbolic acid by treatment with soda. This acid, in a 70 per cent. solution, is employed for the disinfection of basements, cellars, and privies. For the same purposes, use is made of strong solutions of protosulphate and of sesquichloride of iron, to either of which a dilute solution of carbolic acid (10 per cent.) may be added. For clothing and bedding, the preferred disinfectant is a solution of sulphate of zinc, eight ounces in three gallons of water; with or without the addition of one ounce of carbolic acid. Chloride of lime and sulphurous acid are also favourably spoken of.

Relapsing Fever is the subject of an elaborate and very interesting report by Dr. Stephen Smith, upon materials largely furnished by Dr. E. Harris, of the Metropolitan Board. Opening with a full account of the previous history of the disease in different parts of the world, Dr. Smith gives the particulars of its course in 1869–70 in New York. It began in October, 1869, in an "old rookery" on the Five Points. All the tenements and localities, then and afterwards, the centres of origin of relapsing fever, were resorts of vagrants, and were permanently filled by the most degraded of the tenement-house population. In November, the first cases were admitted into the hospitals. At Bellevue, the disease commenced to spread in January; two physicians, several nurses and patients, in immediate contact with those sick with fever, were attacked.

A Relapsing Fever Hospital was opened on Hart's Island in February. In this institution, also, officers and inmates were affected; viz., two of the medical staff, seven nurses and orderlies, and many other attendants—three of whom died. The total number of deaths for the year, from this disease, in New York, was 184; of cases reported, 2181—a mortality of about 1 in 11. Probably a considerable number of cases were not reported.

Professors Clarke, Flint, and Loomis are quoted at some length, upon the clinical history of relapsing fever. We have room to cite only the main points made out by them and Dr. Smith in his careful researches. These are, that the period of incubation is very uncertain; the invasion, almost always abrupt; duration of the first stage, from two to twelve days, mostly between five and seven; that of the second stage, four to eight days; of the period of relapse, from three and a half days to seven. The time of highest temperature is not the same in all cases. In one instance, observed by Prof. A. Clark, it reached 108½° on the third day of the relapse; in another, it stood at 107° for two or three days; both of these patients recovered. Almost every case presented a higher temperature during the relapse than during the primary paroxysm; and in nearly all it fell below the natural standard in the defervescence. In one case it remained at 95½° for two days.

Upon the question of the origin of relapsing fever in New York, Dr. Smith does not seem to be prepared for a decision. He regards its importation as most probable. As to its contagiousness, this, he remarks, "was recognized by even the most casual observer." After reading with care the evidence given of this in the present report, it does not seem to us possible to arrive at any other conclusion. Yet *local* and *personal* influences are manifestly important elements, in accounting for the different degrees of susceptibility. As Dr. E. Harris states, "thirty-eight fever nests" included nearly two hundred of the cases.

Personal predisposition to this fever has everywhere been observed to be connected, in a marked degree, with destitution. "It clearly sought and found its victims, by preference, among that homeless class of people who wander about the city by day, eating the food which they beg from door to door, or pick up in the streets, and who crowd together at night in damp and filthy cellars and unventilated apartments." Individual exceptions occurred, as when

physicians or others were especially exposed to the contagion; but it never showed the slightest tendency to prevail among the better classes.

The sanitary measures employed by the Board to contend with and arrest the spread of relapsing fever were isolation of patients, and the thorough cleansing and disinfection of infected apartments. Of the results, Dr. Smith writes that "thorough cleansing and disinfection were almost invariably followed by a complete arrest of the fever in the tenements thus treated. The experience of the medical officers in the application of preventive measures was most gratifying." The epidemic attained its culmination in the second quarter of 1870, and from that time gradually declined. The highest number of cases (one hundred) was reported April 2; at the same date in 1871, the disease had almost entirely disappeared.

Yellow Fever is made the subject of a very interesting report from Dr. M. Morris, City Sanitary Inspector, including an elaborate discussion of the causation and prophylaxis of that disease by Dr. J. C. Nott, formerly of Mobile, whose long experience in the "fever zone" in the south was wisely called into service by the Board. The occasion of these labours was the occurrence of yellow fever at Governor's Island, in September and October, 1870, with a few cases in the city of New York. These last cases—eleven in number—were, by the Special Committee of the Board, Drs. M. Morris, and J. M. Cornochan (health officer), and G. Ceccarini, all traced directly to Governor's Island. They were either soldiers absent from the island on leave, or nurses, relatives, or friends of those resident upon it, and had evidently contracted the disease upon the island. The committee at once advised the removal of all those sick on the island to West Bank Quarantine Hospital, and placing the island under strict quarantine; "no communication being allowed by any soldiers or residents with the infected quarters, until after frost shall have rendered these restrictions unnecessary." Gen. McDowell promptly gave orders to have these measures carried out—the island being, as a station for recruits, entirely under the control of United States government.

Later (Oct. 8) it was concluded to recommend further action; namely, the entire evacuation of Governor's Island; all clothing, bedding, and other textile fabrics there in use to be disinfected by steam-heat; the old wooden buildings known as the "Camp Women's Quarters," and the "Row," near the South Battery, to be torn down and burned, and their basements or cellars to be filled up. Upon these propositions, a correspondence ensued with Gen. McDowell, whose response, declining to carry them out, is given in full. We apprehend that while the most measured courtesy is, perhaps, apparent on the part of the committee of the Board, the position taken by the military commander is quite as easily reconciled with sound logic, and with his duty to the men under his charge. As to the proposal to vacate the island, he thus writes:—

"Governor's Island has been in quarantine since last Friday week, the most noticeable infraction of it being, as I understand from this report, by the committee of the Board itself, who it seems visited the Island and then returned to the city, a course which, in carrying out what I supposed to be the wish of the Board of Health, I have not suffered any one in the military service to follow. The committee of the Board say new cases are occurring daily, that everything now belonging to the inhabitants should be disinfected, and I have been called upon to keep the island in quarantine, but now the Board ask that, with a few exceptions, all in it should be made to leave it. Some of them, it is to be presumed, have already become infected. Shall these people, some eight hundred men, women, and children, be brought to the city? or shall they be sent inland? What community would, under the circumstances, consent to receive them? There is no other place I know of where they could be sent where they would or could be so isolated as where they now are."

If the Board had been well grounded in its first requisition, that a strict personal quarantine should be maintained at the island, these objections of Gen. McDowell would have been entirely valid; as such non-intercourse can be only justified by the assumption of the *personal contagiousness* of yellow fever; and even the noble self-sacrifice of the committee, in visiting the island, at the risk of their lives, would have been misplaced. But the more deliberate action of the Board was based upon another principle. In its different utterances we cannot fail to see some inconsistency. On one page of the committee's report, when referring to the eleven persons who escaped from the island, or visited it and returned to the city, it is said that "all such persons have been carefully sought for throughout the city by the Health Inspectors, and every possible care taken to prevent any detriment to the public health by *exposure to these individuals.*" (The italics are ours.) And on the same page (p. 343), it is again written that "the removal of the troops and other persons, with the precautions advised, would not, it is believed, convey the disease to any other locality, and might save much sickness and many lives." We remark upon this because it exemplifies a kind of indecision or contradiction not now uncommon, but sometimes disastrous in its results. The committee's report speaks of it as the belief "now universal in the medical profession, that the yellow fever poison is not conveyed from place to place in the person." The conveyance by *fomites*, mentioned as taking the place of the idea of contagion, we may refer to again presently. But it would be well if all the profession were fully agreed upon this first point, most emphatically brought out and strongly supported by Dr. Nott, in his report in this volume; that the special cause of yellow fever, the "germ," as he calls it, "*is not bred in the human body*;" "it is never *personally contagious*; though *sometimes* conveyed by vessels, and *sometimes* taken in trunks, boxes, etc.," from place to place.

The requirement of the Board that Governor's Island should be evacuated was judicious, though tardy. Abundant evidence has been brought in favour of this as a principle. We may cite its declaration as long ago as 1825, by Dupuytren, before the Academy of Sciences in Paris:[1] "We should therefore regard as incontestable the principle which consists of evacuating immediately the places where yellow fever is declared to be, and everything for this purpose should be adopted. The utility of such a measure must always justify its rigorous execution." Sometimes it becomes wearisome to go repeatedly over an old argument. But what else can be done, until a truly *final* consistent conclusion be reached? We hold, with Dr. La Roche, that this finality must include the *total abolition of personal quarantine.* It is *à propos* to cite this distinguished writer's latest utterance on the subject; in his exhaustive report to the Board of Health of this city, "On the Origin and Mode of Progression of Yellow Fever in Philadelphia in 1870."[2] Thus he writes: "It is needless to quarantine individuals arriving at the lazaretto in infected or suspected vessels from sickly parts, whether those individuals be in health or labouring under the fever—the necessity of such measures being disproved." A somewhat hesitating adoption of the same conviction appears in the action of the Philadelphia Board of Health[3] in 1870. "As regards the crew (of the infected brig Home)

[1] Second Report on Quarantine, Gen. Board of Health, London, 1852, p. 204. "From Barcelona, in 1821, about 80,000 persons fled; and excepting those ill on leaving, all were exempt from the malady. Of those remaining in the city, nearly 10,000 died."

[2] Report of the Board of Health of Philadelphia, for the year 1870, p. 85, Appendix.

[3] La Roche, Report, etc., p. 22.

the Board resolved, on the 2d of July, that they be permitted to leave the station with the consent of the lazaretto physician." This consent being withheld, the Board again resolved, July 13th, "that the stevedores, and the crews of the lighters engaged in the discharge of the brig Home, be permitted to come up to the city, if in the opinion of the lazaretto physician it can be done in safety to the inhabitants." It would seem that this last condition, attached to the Board's resolution, along with the prepossessions of the physician, Dr. Thompson (himself soon after a victim to the disease), may have cost several lives. Dr. H. E. Goodman[1] states that, of those who took directly to the city the first barge that received a cargo from the Home, no one was attacked; while, after the date of the second resolution of the Board (July 13), four persons died, all of whom were connected with the barges near the infected vessel. We would express our conviction upon this subject in the emphatic language of Dr. Reese of New York[2], uttered many years since:—

"I am of opinion that the oppressive features of our quarantine system should be reckoned among the relics of barbarism which an enlightened legislature should make haste to abrogate for the sake of our character as a people. There is no pretext for the perpetuation of a system founded in ignorance, and fruitful only in public and private injustice, cruelty, and wrong."

When all sanitary authorities shall agree to insist upon the most *rigorous inspection* and *effective purification* of every *vessel* coming from the yellow fever zone during warm weather, and (while the idea of *fomites* is still entertained by some), the proper disinfection of all movable goods and cargoes—the removal of all persons from infected vessels will tend to *expedite such* measures, and thus to prevent the concentration of the poison. Towards such a conclusion, the able treatise of Dr. Nott in this volume is an important contribution; as it draws so clear and strong a distinction between *contagion* and *portability*, and affords most cogent evidence against the former being an attribute of yellow fever. At the same time the views of Dr. Nott do not appear to be all quite unassailable; especially in regard to the existence of two *species* or types of yellow fever, the one portable and the other not.

Many other subjects, besides those we have referred to, might, as they are dealt with in this report, detain our attention longer. But we must close with brief reference to one of them alone. On *Sanitary Engineering*, two reports are presented, by Egbert L. Viele and Chas. H. Haswell. Both dwell upon certain evils connected with the growth of the city of New York, the prevention of which should be a primary object in every municipality. Says E. L. Viele (p. 413):—

"One of the chief causes of mortality is to be found in the defective drainage of certain districts of the city; and this is an evil which is mainly increased by the so-called city improvements, or grading of streets and avenues which are now being carried forward.

"Geologically the island of New York is a metamorphic granite or granitic gneiss, whose originally horizontal planes of lamination have been disturbed and thrown up. . . . . An extremely irregular surface has thus been formed, with numerous valleys, through which originally flowed many drainage streams, receiving their supply not only from the surface drainage but also from innumerable perpetual springs percolating through the planes of lamination. . . . There are plague-spots and fever-nests which exist in the vicinity of old obstructed drainage streams, which no sanitary regulations can eradicate. . . . . Every day the progress of population, the extension of

[1] Report of Phila. Board of Health, 1870, pp. 59, 60.

[2] Evidence before the N. Y. State Committee on Quarantine; Albany, 1845.

streets, and erection of buildings, increase the obstacles which have to be removed, and will in a short space of time be insurmountable, except at an enormous expense. . . . . It is important that the avenues of communication should be laid out in accordance with a harmonious general plan. Unless a comprehensive system is established in advance of private improvements, it becomes a matter of great difficulty and sometimes an impossibility to correct the evils that may arise."

We have been able to do, we fear, but scanty justice to the valuable papers contained in this Report; but the limited access of readers, generally, to such documents makes it desirable to place some of its most important matter before the profession. We know of no better *data* for the study of practical public hygiene. H. H.

ART. XXIII.—*Earth as a Topical Application in Surgery. Being a full exposition of its use in all the cases requiring topical applications admitted into the men's and women's surgical wards of the Pennsylvania Hospital during a period of six months in* 1869. By ADDINELL HEWSON, M.D., one of the attending Surgeons to the Pennsylvania Hospital. With four Photo-relief illustrations. 12mo. pp. 309. Philadelphia: Lindsay & Blakiston, 1872.

How best suppuration can be prevented, or at least restrained, is a problem that during the last ten years especially has engaged the attention of practical surgeons; and with good reason too, for the comfort of the patient, and still more the life of the part, if not of the whole, is involved in its solution. Suppuration being always the result of over-stimulation, the great desideratum is a dressing that shall prevent irritation; and as in the majority of cases it is external influences that unduly stimulate, what is most usually necessary is the protection of the wound against such disturbing causes, be they chemical or mechanical. Is it the air itself that irritates, or living organisms in the air? The question is unsettled and surgeons have shaped their practice according to their beliefs. Maisonneuve, with his rubber cap, would exclude the air altogether; Lister, with his carbolized gauze, destroy the living organisms before permitting the air to reach the wound; Guérin, filter the air of such organisms by passing it through cotton; some by the topical application of an albumen-coagulating agent form a pellicle "prohibiting the germinal cells below the stratum of coagulum from the deleterious action of the atmosphere, just as the epidermis protects the true skin," and this on either the oxygen or germ theory.

Dr. Addinell Hewson, recognizing the value of earth in the disinfection of animal excreta, has for three years been testing its value as a surgical dressing, and in the monograph under review has given the details of his first series of cases, 93 in number; "all the cases requiring topical application admitted in the men's and women's wards of the Pennsylvania Hospital during a period of six months in 1869."

"I have been compelled to confine myself on the present occasion to an exposition of my first six months' experience only. I have not done this, however, without some satisfaction, for my inferences of those times have not only been thoroughly confirmed by more recent experiences, but there were numerous and constant witnesses of the cases, and by giving the details of *all* of them, I avoid the suspicion of suppressing anything which could have then led me to other conclusions, and they furnish the fairest means of judging of the correctness of the views which I now hold."

These 93 cases embrace all that variety of surgical diseases and injuries which might be looked for during a half-year in the wards of a large city hospital, long and justly famous for its surgery. Ulcers, lacerations, fractures, amputations, burns, cases likely to heal under any dressing or no dressing, cases of doubtful prognosis, cases necessarily mortal, each and all were subjected to the earth treatment. Earth was dusted over the surface, packed into the orbital cavity, rectum, and vagina, plastered over the face. Of the 93 cases 14 died, and of the remaining 79, 10 seemed to have received no special benefit from the earth application, and 11 were either removed from the hospital before cure or transferred to the surgeon next on duty, the earth dressing in either case being abandoned. In some of the remaining cases primary union or unusually early cicatrization took place, in others the recovery was as tedious as under the old and well-known methods of treatment. In his "*comments on cases*," Dr. Hewson writes:—

"A careful examination of the details of all these cases will, I think, satisfy any one that certain phenomena have had place after the use of the earth as a topical application, which, at least in the majority of them, may be fairly considered either as its direct effects, or as results materially influenced by it. These may be arranged as follows:—

I. Effects as to the contact of the earth with the parts.
II. Effects on the pain naturally incident to the cases.
III. Its power as a deodorizer.
IV. Its influence over inflammation.
V. Its influence over putrefaction.
VI. Its influence over the healing processes.

"I. The effects as to contact may be well considered as those which are primary and those which are secondary." (p. 188.)

The "primary effect" is unirritating, "cool and pleasant," being the "prompt answer of the patient constantly given," burning pain being experienced only when hemorrhage had taken place under the dressing.

"It was impossible to determine in a distinct and positive manner the existence of any secondary sensations or irritations dependent on the mere contact or presence of the earth, so as to distinguish them from those which we will consider under the next head, namely, the pains naturally incident to the morbid state or action in the part, unless we should except one, and that was, where we used a pure article of clay which did not allow of free absorption of the discharge, but, on the contrary, formed, by caking, a barrier for the escape of the discharge, and so occasioned pain; or, where the earth of a loamy character was not abundantly applied, and crusting the margins became the source of pain, by being pressed into the part, under such circumstances.

"II. In the changes produced by the earth on the pains incident to the cases themselves, there were in some instances most positive and unmistakable evidence of a favourable effect; whereas in the others evidences of such an effect were wanting, or of a negative character, and in some the earth appeared to aggravate the pain. . . . But absence of pain has characterized all kinds of wounds among these cases, whether great or small, under the dressing, save where secondary hemorrhage had taken place, and was indicated by a 'burning pain.' In burns or scalds of the first degree, in erythematous inflammations, in ulcers where the pain was in the superficies, and there was no periosteal complication or constitutional taint, we had always a distinct or marked abatement of suffering following the earth's application, and the relief continued even where there was an abundant discharge of pus, as long as there was no penning up by the earth. In cases of burns or scalds attended by vesication, or of wounds where a bad article of collodion was used in the dressing, complaints were not rare shortly after the earth was put on, and points were then indicated as the seat of considerable pain, where we found afterwards blisters

to have been formed." These blisters contained "fibrinous or coagulated lymph. Such lymph is of rare occurrence in similar cases amongst the patients in our hospitals where other dressings are used, and hence I am disposed to attribute its occurrence here to the presence of the earth. Lymph of this kind would seem from my observation to occasion more pain than the serous form, when pent up under a vesicated cuticle." (pp. 190–1.)

The *deodorizing* effect of the earth (as our author prefers to call it rather than *disinfecting*) was well established by the cases under treatment, and also by experiments made in the dissecting-room of the Jefferson Medical College.

"IV. *Inflammations.*—In no instance was there any evidence of the earth either provoking or aggravating inflammatory action. In those where such trouble was present but deep-seated, or dependent upon taint, it had no appreciable effect; but in all the other cases its use was attended by a sedative and beneficial influence in this respect. In some it retarded such action, in others it appeared to arrest it, and in others it actually prevented its development.

"Case No. XI. is an interesting case as showing the effects of the earth in all these respects. . . .

"Over the development of the abscess there was no evidence of the earth having exerted any influence; but it clearly prevented its exciting any amount of inflammation towards the surface in the surrounding parts. . . .

"Many other cases in the series, of a minor character, might be referred to, but it seems hardly necessary. They all, however, justify the claim for the earth of a power to prevent, and a positively soothing influence over inflammatory action. This I would claim for it, with the recollection and full consideration of the effect from caking pointed out before, for that, as I have mentioned, was a defect in the *method* of using the article, and not necessarily inherent in it as a dressing.

"V. *Putrefaction.*—The efficient manner in which the prevention of this process was accomplished, can be judged of by the details of every one of the cases, in which we had any reason to anticipate such action here referred to. And as evidence of a negative character, I would draw attention to the difference in results in those cases where I abandoned the earth dressings for poultices, and thereby got a rapid separation of the dead parts, with greatly increased discharge, and offensive odour. In the case of C. B. (XLIX.), where I attempted a Pirigoff through injured tissue, and had the misfortune to wound the posterior tibial, as also the artery running down alongside of the tendo Achillis, which I had to divide on account of the violent action of the muscle, and the posterior flap died from being thus cut off from its chief source of nourishment, I had the opportunity of watching the effects of the earth in this respect. Although perfectly dead, the flap seemed not all disposed to separate from the living parts, or in itself to become putrid. In fact I trimmed away, from day to day, the portions which were undoubtedly dead, but never could ascertain when it was going to form its line of demarcation until I had resorted to the influence of a fermenting poultice, which caused its putrefaction in a most extraordinary manner. . . . This extraordinary increase of purulent matter continued as long as I used the fermenting poultice, namely, for twenty-four hours after all the slough was got away, when I again resorted to the dry-earth dressing. . . .

"VI. The last is by no means the least important or the least significant of the results which I have to record, but it needs the least comment of all to attract attention to its occurrence. The singular rapidity with which healing took place in the cases of wounds, whether from operations or accidents, of which I have given the details, and similar effects, so noticeable in all the ulcers, except where there was a local cause, or a constitutional one, must have struck the reader on their perusal as presenting evidence of a remarkable power in this respect." (pp. 196–9.)

As respects the "remarkable power in this respect," we cannot but feel that a most natural enthusiasm has led Dr. Hewson to see more evidence of the

existence of this power than others will be able to do. Widely extended trials of the dressing will be required before any other than the Scotch verdict of "not proven" can be given.

One-third of the book is devoted to the consideration of the *modus operandi* of the earth-dressing, the setting forth of the author's views as to the *how* and *why* the stated results followed the applications. Carefully prepared, evidently the product of much labour and thought, of no little scientific interest and value, we very much doubt if it will be enjoyed, even if read, by one in twenty of those who, having confidence in the author's skill and honesty, will turn to the book for information and practical suggestions. Except for a very few there is altogether too much Agricultural and Physiological Chemistry, too much that will repel rather than attract readers. Unfortunately, to medical men in general, Chemistry, except in its most elementary form, is "an unknown terrible." Dr. Hewson should at least have added a final brief chapter, giving a *résumé* of his theories and beliefs. "Is it merely by purely physical properties, such as excluding the air, absorbing the moisture and the excess of discharges, and affording uniform compression and support to the parts, that the earth can be of any benefit?" The author thinks not, for "the most thorough comparison of all these earths" (sand, clay, humus), "by their physical properties alone, and their effects as dressings, show no correspondence between such properties and effects." Is it then "from any chemical influences which it can exert?" So he would have us believe; these chemical influences consisting in the power of earth to oxidize and deoxidize, to decompose certain salts and absorb and retain their bases, and to effect the substitution of *ammonia* for certain other bases (*potassa, soda, lime, magnesia*), of which *potassa* when set free has an influence in promoting the healing process.

However much the record of cases detailed may fail to convince any one of the reality of the full value claimed for the earth-dressings, and however much one may be inclined to question the correctness of the theories adopted, and consequently of the given explanations based upon such theories; yet, the publication of Dr. Hewson's interesting monograph will not only add to its author's already excellent reputation as a surgeon and professional worker, but will induce many to make thorough trial of the application of earth to wounds.

The four illustrations (Woodburytypes) are interesting in themselves, and of positive value as representations of the cases. The excellence and cheapness of reproductions of photographs by the Woodbury and Albertype processes will, it is to be hoped, secure for us in the future better, even if fewer, illustrations in our medical books. P. S. C.

ART. XXIV.—*Injuries of Nerves and their Consequences.* By S. WEIR MITCHELL, M.D., Member of the National Academy of Sciences, Physician to the Philadelphia Orthopedic Hospital and Infirmary for Diseases of the Nervous System, etc. etc. 8vo. pp. 377. Philadelphia: J. B. Lippincott & Co., 1872.

IT would be a great gain to physiology and to scientific medicine, if all who have had the enjoyment of special opportunities for studying groups of important cases felt, as does Dr. Mitchell, that they owe some account thereof to the profession, and the gain would be greatly increased if all such were as able as he to render so good an account of their advantages.

Dr. Mitchell is already known as a worker in this field, by his authorship, in connection with Drs. Morehouse and Keen, of a work on "Gunshot Wounds and other Injuries of Nerves: Philadelphia, 1867," now unfortunately out of print, of "Circular No. 6" (March 10, 1864), from the Surgeon General's office on the same subject, a paper in the medical volume of reports of the Sanitary Commission, and various other articles.

It is to be regretted that a larger portion of some of the former works is not embodied in the present for the convenience of those who, like the writer, have tried in vain to obtain them.

The experience which formed the basis of the works just mentioned and, increased by many cases, of the present one also, being derived from the Turner's Lane Hospital, which collected into its four hundred beds cases of nerve injury from the general hospitals of the eastern military departments, was probably, and we hope will long remain, entirely exceptional in extent, giving to the statements of this work the highest authority in regard to classes of cases which must be, in any ordinary practice, among the rare exceptions.

The usefulness of the book, however, is not limited by its consultation in these rare cases, since it contains many pathological and therapeutical data of much wider applicability.

In the second chapter, upon the anatomy of nerves, some curious facts are given in regard to the amount of stretching which may be borne by them without loss of function. The sciatic nerve of a rabbit was found to conduct electrical excitations, when stretched to the extent of three-quarters of an inch in three inches. A sciatic nerve, in a case of hospital gangrene, was roughly drawn out of the cavity by the removal of the dressing and then thrust to the bottom or one side to the extent of four or five inches. "Some pain resulted, but the sense of touch was hardly disturbed." Nerves, like arteries, frequently escape destruction by being thrust to one side by the passage of a ball.

The chapter on physiology contains much interesting matter in regard to the vexed question of "trophic" nerves. Dr. Mitchell does not consider the vaso-motor function as sufficient to account for the nutritive changes taking place after injuries to nerves, especially when they occur, as sometimes happens, without material change in the temperature or colour of the part. The existence of trophic nerves, as distinct from motor and sensory, he does not, however, consider proved. His experience seems to coincide with that of Brown-Séquard in regard to the occurrence of remote lesions rather in connection with nerve irritation than with paralysis. He does not agree with the observer just mentioned in assigning a special set of conductors to each class of sensations.

In the next chapter, after some remarks upon experimental pathology, a branch of inquiry to which Dr. Mitchell is well known as a valuable original contributor, we have accounts of inflammation, acute and chronic, degeneration and regeneration of nerves. The treatment recommended for the former affection, fortunately not common, is cold (ice and ice water), rest, morphia, and atropia.

The multitude of instructive facts and pathological deductions therefrom, in the next chapters, upon "Varieties of Mechanical Injuries," "Symptomatology" "Remote Symptoms," and "Sensory Lesions," is so great that we can only briefly indicate some points of special interest. Such seem to us to be the experiments upon the amount of pressure necessary to cause paralysis, in connection with the bruising of nerves by missiles, and their injury from tumours, dislocations, reduction thereof, the fœtal head, obstetric forceps, crutches, and other sources of compression. The propagation of pain, paralysis, and anæsthesia

from a wounded nerve to others arising from the same plexus, is attributed by the author to a centripetal extension of the inflammation involving neighbouring branches. He takes occasion to object, as he has before justly done, to Brown-Séquard's theory of reflex paralysis from vaso-motor spasm. Some of the most interesting passages are those relating to the severe burning pain or "causalgia" sometimes accompanying nerve lesions and very likely to coexist with or precede the "glossy skin." The fact that the burning pain was lessened when cutaneous vesicles appeared, as they frequently did in successive crops, may be of interest to those who disbelieve in counter-irritation on theoretical grounds.

The effect of severe pain upon the mind is forcibly described in several passages and illustrated by cases:—

"Under such torments the temper changes, the most amiable grow irritable, the soldier becomes a coward, and the strongest man is scarcely less nervous than the most hysterical girl." Of a man who suffered from acute neuritis he says: "From being a man of gay and kindly temper, known in his company as a good-natured jester, he became morose and melancholy, and complained that reading gave him vertigo, and that his memory of recent events was bad."

The subject of nerve suture is discussed, but the author reports no case of his own where it was employed.

Electricity, of course, claims considerable attention, being of importance not only therapeutically, but as a means of prognosis. Manipulation, or "shampooing, rubbing, massage," etc., is a means of treatment highly prized by Dr. Mitchell. It is to be regretted that so efficient a therapeutic resource should be neglected by physicians and suffered to fall into the hands of quacks.

The experience of the Turner's Lane Hospital in hypodermic injections at a time when they were much less used than at present, was an extensive one. "It was usual at one period, I believe, for the assistants to give every morning and every night between sixty and eighty hypodermic injections. During one year at least forty thousand doses of various narcotics were thus administered without any accident."

"I had large opportunity for studying in traumatic neuralgia the comparative merits of narcotics used by the mouth or under the skin, and I have no hesitation in stating as my opinion, that the latter method is not only the more efficient, but also the less harmful constitutionally."

Counter-irritation in various forms seems to have been found of no doubtful utility.

Neurotomy, with a table of cases, and the treatment of remote lesions of nutrition, occupy Chapter XII.

Chapter XIII. contains several cases of wounds of special nerves, the sympathetic and cranial nerves, which are of high physiological interest. That of wounded sympathetic is, we believe, unique. A point of interest therein is the fact that although the symptoms corresponded, in many respects, very closely to those observed after experimental section of the nerve, and one side of the face became much flushed, the retina presented no abnormal appearances.

Some remarks on the pathology of stumps are well worth attention, though the facts adverted to are not new. Such for instance as those upon the various sensations of the presence of a lost hand or foot, and upon the influence of the weather in enabling some patients, like those with chronic rheumatism, to predict an easterly storm.

If, in thus briefly pointing out a few, and only a few, of the instructive and interesting passages in which the book abounds, we shall induce any to give it

the careful study it deserves, not only from the specialist, but from the general practitioner, who cannot fail to receive therefrom suggestions far beyond the range of immediate applicability, we shall have done a real service to them and to scientific medicine. R. T. E.

ART. XXV.—*Plain Talk about Insanity: Its Causes, Forms, Symptoms, and the Treatment of Mental Diseases, with Remarks on Hospitals and Asylums, and the Medico-legal Aspect of Insanity.* By T. W. FISHER, M.D., late of the Boston Hospital for the Insane. pp. 97. Boston: Alexander Moore, 1872.

WE welcome this modest little book as one which will exert a most wholesome influence. While it is especially intended for the general reader, physicians not practically acquainted with insanity may derive most valuable hints and sound doctrine from its pages. If by chance it should be opened by any lawyer, who may be not perfectly satisfied that he already possesses the full sum of human wisdom concerning mental disease, he may learn therefrom, if he will, lessons of unusual truth and soberness. Possibly he may henceforth venture to doubt the infallibility of the judicial mind that pronounced the dictum that upon questions of insanity the opinion of any man of common sense is as good as that of an expert.

The following is an abbreviation of the table of contents: Causes, Forms, Symptoms, Partial Insanity, Monomania, Medical Treatment, Moral Management, Home Treatment, Hospital Treatment, Hospitals and Asylums, Medico-Legal Aspect of Insanity.

The earlier chapters give in a style comprehensible to the general reader much information as to the nature, varieties, causation, and—indirectly—prevention of insanity. Perhaps, however, the chief value of the work lies in its lucid and forcible exposition of the preëminent advantages of hospital treatment of the insane. In a manner as unassuming as convincing, Dr. Fisher shows the superiority of the hospital over the home, the boarding-house, or travel, as a means of promoting the safety, comfort and recovery of the insane. He graphically exhibits the folly and unreason of the prejudice and distrust as to hospitals and their managers. Appreciating the natural reluctance of friends to consign their afflicted ones to the care of strangers at the very time they seem most to need the ministration of loving hearts and tender hands, he points out how the once happy home and beloved family become often the most irritating of all influences; and how, even when the affections are not perverted, the very solicitude of anxious friends and the familiar surroundings of the home, serve to aggravate the disease.

The chapter on Partial Insanity gives an extremely clear account of the manner in which mental disease may be limited, either in its degree, as affecting the faculties with greater or less severity, or in its extent, as affecting solely or principally certain portions of the intellect or certain emotions of the heart. The misery caused by this form of insanity is forcibly depicted and illustrated. The fact that the public at large do not generally recognize the existence of such disease increases immensely the difficulty of protecting and caring for its subjects, and of securing the comfort and safety of their families. The picture Dr. Fisher paints of the part taken by disinterested neighbours, in opposing and interfering with the measures deemed necessary by the family, and sanc-

tioned by their physicians, in regard to a "partially" insane person, is, he tells us, taken from life. This we can well believe, for such instances are of frequent occurence. Ninety-nine per cent. of the sensational stories of "incarcerations" of sane and virtuous husbands by wicked wives, and of faithful loving wives by designing and unfaithful husbands, which pass from newspaper to newspaper, from Maine to Texas, arise simply and solely from limited mental disease which the public, ignorant of the facts, or blinded by prejudice, do not recognize. Dr. Fisher notes the important fact that the victims of partial insanity often exhibit their disease almost exclusively in their homes and to their families; conversing and behaving with much self-control and propriety in the outer world. This should be known and remembered, and we should be cautious how we stigmatize as wicked conspirators against their own flesh and blood, men and women who have always shown themselves honest and honourable. Yet how often this is done, and what a singular phase of human nature it reveals!

While, in the circumstances we have been supposing, the family of the patient are the chief sufferers, in another course of events the unhappy subject of disease may incur disgrace, degradation, even death, for acts which result from his malady. Let him under the rule of some jealously concealed delusion, or from the promptings of diseased impulses, or from total absence of moral power, become the doer of some terrible deed of blood,—and behold, the very public who had protested against his restraint, shall be found clamouring for his punishment as a depraved and monstrous villain.

In the brief chapter on Medical Treatment, attention is earnestly called to the importance of recognizing and at once treating the first signs and warnings of threatened insanity. By insisting on immediate rest or change of work; or by procuring sleep; or equalizing disordered circulation; or invigorating an enfeebled stomach; or by toning and stimulating exhausted nerves,—attacks may be prevented.

The chapter on Moral Management is full of sagacious remarks which show how fully the writer has improved his means of observation. In concluding this subject, Dr. Fisher reminds us that there is always a considerable proportion of the insane who are insusceptible to moral influences. In some, the moral sense is either wanting or perverted; in others, cerebral excitement is too violent, or dementia too complete, to allow of any response to appeals to the feelings.

In discussing the question of placing a patient in a hospital, the author argues that mere feeling upon the matter, either of friends or patient, should be allowed no weight. The grave responsibility rests on the physician to counsel and on the friends to do, what the best medical opinion dictates. This responsibility cannot be evaded. The writer, however, very justly remarks that even in cases where hospital treatment is undoubtedly needed, there may be no harm but a positive advantage, in letting the patient remain at home for a short time. The result of this delay often is to fully convince the family, by their own experience and observation, of the wisdom or even necessity of resort to the hospital. Thus they will be more likely to persevere in this treatment than if not fully convinced of its needfulness.

A few classes of cases are mentioned which may with propriety be kept at home. Dr. Fisher omits to mention among these the rapidly fatal form of disease known as typho-mania. The cases he mentions are some of the less violent, incurable and senile sort, and some others which seem dependent on physical causes susceptible of speedy removal by medical treatment. We believe the doctor errs in including any cases of epileptic mania in the number

of those proper to be treated at home. Such patients are never safe out of hospitals.

The chapter on Hospital Treatment contains some sharp hits at the visionary schemes of self-styled reformers, and at the narrow views generally taken by the public of all matters relating to the insane and their guardians. Those who clamour for "free air" and "family life" are quietly reminded that it was during the possession of these blessings that the patients became insane.

Self-control being impaired in the insane, the hospital with its skillfully regulated restraints supplies its need and encourages its recovery. Attempts at restraint are generally resented in the patient's own home; but in the hospital are found the controlling influences of a novel, regular, fixed course of life, new surroundings, new acquaintances, and the example of all, sane and insane, conforming to the discipline of the house. Often, the brain needs, above all things, rest. In this case the hospital is a sort of "mental fracture-box." "To others, and at times to all, it is a mental gymnasium," affording exercise to certain faculties and rest to others. "The manifestations of insanity usually concern the affairs of every-day life, of business or the family. The disturbed feelings and delusions are in intimate relations with the persons and things the patient has been daily accustomed to see." Hence, the advantage of the hospital, free from all old associations.

Dr. Fisher is justly emphatic in warning his readers of the perils that attend convalescence from insanity. These arise from ignorance of the irritable state in which the brain is left for a considerable time after apparent recovery. The hospital physician has occasion in scores of instances to lament the consequences of premature removal. The patient appears sane, and urges his friends to remove him. Often he insists that remaining, after recovery, among the insane, will make him crazy. Then the relatives think that the cure may be all the more speedily confirmed by the cheerful influences of home; forgetting that it was among these that the attack began. Their feeling is as natural, and as harmful if indulged, as the idea that a typhoid fever patient will do better on solid food because he begins to crave it. Complete relapse often results, and with it a diminished hope of recovery. When actual relapse is escaped the patient often retains through life some traces of his disease, which would not have remained under wiser treatment. It is an instructive fact, often remarked by the hospital physician, and referred to by Dr. F., that very often a patient who has been clamourous for an immediate discharge, becomes, if detained a few weeks longer, much less self-confident. He realizes more fully his past condition, is willing to do anything to secure future sanity; and even dreads to leave the quiet haven where peace and reason have come back to him, and to subject his new-found health to the rude tests of a busy world.

In a sensible chapter on Hospitals, Dr. Fisher recognizes the utter impracticability of the "Cottage Plan" as a means of *properly and cheaply* caring for the insane. The "Colonization" system of England and Scotland he says is little different from the practices formerly prevalent in some parts of our country, of "farming out" the poor to the lowest bidder. Certainly, accounts given us of facts in Scotland fully warrant the comparison.

The chapters on the Medico-legal Aspects of Insanity are admirably adapted to the enlightenment of the popular mind. Legal gentlemen, too, are delicately reminded that physicians who have devoted their lives to the study of insanity, in daily intercourse with hundreds of patients illustrating all forms and degrees of mental disease, may, without extravagance, be presumed, until the contrary is proved, to know a little more about such things than the rest of mankind,—

even than advocates and judges. There are few matters in which the proverbial conservatism of the law is more hurtful than in its relations with insanity. The decisions and charges of many courts utterly ignore the scientific advances of the last two centuries.

In this connection we find a short but clear description of "moral idiocy," with well chosen illustrations. The hereditary causation of many of these cases is pointed out. Some famous trials, of our time, are adduced in evidence of the popular blindness and incredulity as to the reality of this form of congenital deficiency. As a contrast to the picture of poor, irresponsible wretches, visited with the direct vengeance of the law, another set of cases is mentioned where popular sympathy has countenanced and juries admitted, the most baseless pleas of insanity, thereby setting free, unpunished, deliberate murderers, to walk among men with confident step and uplifted head. Transitory mania undoubtedly occurs. Probably many a poor young mother, guiltless of crime, is condemned for infanticide; certainly many a popular or influential assassin now enjoys liberty and the respect of men,—both wrongs being due to want of knowledge and proper appreciation of momentary, impulsive, homicidal madness.

Dr. Fisher again exhibits the evil results of the curious popular distrust and prejudice against all who have occasion to bring forward an allegation of insanity, as seen in case a family tries to protect itself from a member who is becoming insane. He may be squandering his property, incurring ruinous obligations, or threatening murder or suicide. Yet, too generally, public opinion and the law will not consent to his restraint until he has become a beggar or a homicide. Truly, personal liberty should be a precious jewel, to be guarded at such cost!

In litigation concerning wills, contracts and obligations, the little knowledge of insanity possessed by lawyers and the public is fully as apparent as in the criminal cases referred to. In illustration of this branch of his subject, Dr. Fisher gives a number of cases which have gained a wide notoriety through the newspapers. Could his sensible and intelligent comments reach all who have devoured the sensational reports and untrue representations of the newspapers, we should hope before long for a more healthful public sentiment.

B. L. R.

ART. XXVI.—*A Treatise on the Diseases of Infancy and Childhood.* Second edition, enlarged and thoroughly revised. By J. LEWIS SMITH, M.D., Curator to the Nursery and Child's Hospital, New York; Physician to the Infant's Hospital, Ward's Island, etc. etc. 8vo. pp. 741. Philadelphia: Henry C. Lea, 1872.

IN a review of the first edition of this work, which appeared in the number of this Journal for April, 1869, we expressed the opinion that while it was excellent in most respects, it could not be considered to be a complete treatise on the diseases of infancy and childhood, inasmuch as no mention was made of many of the more important as well as the more common of the diseases of children. Dr. Smith has done much towards removing this defect in the edition before us, and the book has, consequently, been rendered much more valuable. Nearly twenty additional diseases are treated of; among which may be mentioned diseases incidental to birth, rachitis, tuberculosis, scrofula, intermittent, remittent, and typhoid fevers, and the various forms of paralysis. From the preface we also learn "that many new formulæ which experience has shown to

be useful have been introduced, portions of the text of a less practical nature have been condensed, and other portions, especially those relating to pathological histology, have been rewritten to correspond with recent discoveries." In consequence of these alterations, and notwithstanding an increase in the size of the page, it has been found necessary to enlarge the volume by the addition of more than one hundred pages.

The disparity which we pointed out in our notice of the first edition of Dr. Smith's book as regards the space given to the discussion of different diseases still exists, but in a much less degree than before. The chapters on diseases of the respiratory organs have been rendered very much fuller, particularly that on pneumonia, which has been rewritten, and is in accord with the views of the disease held by the most distinguished pathologists. Under the head of cerebro-spinal diseases we find that very little change has been made, except of course, in the addition of chapters on chorea and the various forms of paralysis. Dr. Smith while fully recognizing the distinction between simple and tubercular meningitis, and admitting the possibility of distinguishing between them during life, still persists in treating of them in the same chapter, which we cannot but regard as a mistake. The chapter on syphilis in the present edition contains an allusion to the peculiar malformation of the upper incisor teeth and to the interstitial keratitis which are so frequently found in the subjects of congenital disease, and to which no reference was made by the author in the first edition of his work. We are rather surprised, however, at the statement that these observations of Mr. Hutchinson need corroboration. We have certainly ourselves often met with children who, in addition to having a history of syphilitic disease, presented these as well as other signs of syphilis, and were under the impression that they were universally looked upon as important aids to diagnosis.

The chapters on tuberculosis, scrofula, and rachitis are important additions to the work. Scrofula and tuberculosis are shown to be related, but not identical conditions, while rachitis is believed to be an entirely distinct disease, and is not apt to occur in those predisposed to the other two diseases. We think our author is in error in speaking of the tubercular diathesis, and in saying that it may be inherited. It is evident that he only means the predisposition to it may be transmitted. Adopting, as he does, the views of several German pathologists that tuberculosis is a secondary disease following phthisis or scrofula, and dependent upon the absorption of the caseous products of low forms of inflammation, it is difficult to understand how Dr. Smith can regard it as a diathesis in itself. The term scrofula is applied to a condition which is characterized by increased vulnerability of the tissues; or, in other words, by a tendency to inflammation of a chronic or subacute type, the products of which are very apt to undergo caseous degeneration. Hence the frequency of catarrhal pneumonia and of enlargement of the lymphatic glands in the scrofulous, and one or other of these inflammations will, we think, generally be found to have preceded the deposition of tubercle. In regard to the contagiousness of scrofula, he says, that there is no foundation for the popular belief that the disease is communicable. The manifestations of the diathesis which are sometimes noticed after vaccination are frequently cited as proof that it is so; but the author shows that in these cases vaccination simply has called into activity a predisposition which had before been latent, and is no more a cause of the general disease than measles or hooping-cough, attacks of which will sometimes precipitate the occurrence of similar accidents in persons of like tendencies.

The chapters on diseases of the umbilicus, umbilical hemorrhage, conjunctivitis of the new-born, and accidents and diseases incidental to the birth of the

infant and the detachment of the cord, are also new, and will be found to contain the necessary directions for the management of these diseases and accidents.

The fevers are rather summarily dealt with, only fifteen pages being given to the consideration of intermittent, remittent, and typhoid fevers. The disease, which some writers on children's diseases have described as infantile remitting, Dr. Smith believes to be nothing more than typhoid fever, which in the young, and especially during the first week, is more apt to be marked by evening exacerbations than in the adult. Remittent fever, when it does occur, is always of malarial origin.

We see no reason to qualify the favourable opinion which we expressed in our previous notice of this book, and can confidently recommend it to any one in want of a good hand-book on children's diseases.

The author has altered, to some extent, the arrangement of subjects—and we think that the changes have generally been judicious. The publisher has also added to the value of the book by increasing the number of illustrations.

J. H. H.

ART. XXVII.—*Lithotomy and Lithotrity.* Illustrated by Cases in the Practice of GURDON BUCK, M.D., Visiting Surgeon to the New York Hospital and Presbyterian Hospital, etc. 8vo. pp. 59. New York: William Wood & Co., 1872.

THIS pamphlet gives the histories of fifty patients treated by Dr. Buck for vesical calculus: three by lithotomy alone, five by lithotomy after the failure of lithotrity, and forty-two by lithotrity alone. Nine of the patients died, eight after lithotrity alone, and one after the employment of both methods. The following are the conclusions to which the author's experience has led him with regard to the choice of operation:—

"1. For patients under 17 years of age, lithotomy should be preferred. . . The only exception admissible to this rule might be a case not under ten years of age, in which a stone was ascertained, by measurement with a lithotrite, not to exceed one-half to three-fourths of an inch in diameter; and which might therefore very probably be gotten rid of by a single operation.

"2. For adults, lithotrity is most advantageously employed when the patient's condition is such as existed in the cases comprised in Groups I. and II. of this article, to wit: A moderate size calculus coexisting with a favourable condition of the urinary organs and general system; also where a like favourable condition of the local organs and general system coexists with a calculus of large size but not of hard consistency.

"3. If a calculus be found by the lithotrite to be very hard, and to measure one inch or more in diameter, though at the same time other favourable conditions may coexist, lithotomy should be preferred, as affording the patient the best chance of a good result.

"4. Great difficulty in passing the neck of the bladder with the lithotrite, whether from enlargement of the prostate or from a fixed position of the stone itself, should deter from the employment of lithotrity.

"5. In a debilitated or reduced state of the system from purulent cystitis and protracted suffering, irrespective of the size of the stone, lithotomy should be preferred. Emptying the bladder instantaneously of its foreign contents, and putting it at rest by draining off the urinary secretion, will afford the patient in such a condition the best chance to rally and recover.

"6. In a case of stricture of the urethra, its complete cure should be a preliminary step to the employment of lithotrity."

This record of Dr. Buck's cases (many of which are of much interest) is adorned with several well-executed illustrations. J. A., JR.

ART. XXVIII.—*An Introduction to Pathology and Morbid Anatomy.* By T. HENRY GREEN, M.D., London. 8vo. Philadelphia: H. C. Lea, 1871.

WE cordially welcome this little work as an attempt to fill what has too long remained a sensible gap in English medical literature. At present the work of Paget is the only extended treatise on pathology which at all pretends to be on a level with our knowledge of the subject, and this, far from covering the whole field, only treats of "Surgical Pathology." The work, as the author informs us in his preface, is strictly elementary, and is evidently from the pen of one well versed in the subject of which he treats. It is the most clear and condensed statement of the modern views on pathology with which we are acquainted, and as such we cordially recommend it to all those who have not the time or inclination to study other and more complete manuals. The author is largely indebted to Rindfleisch, both for his illustrations and for the arrangement of his material. At the close of the work is a list of the principal authorities referred to. They are twenty-one in number, and it is indicative of the low ebb at which pathology stands in England that out of this number only five English authors are quoted. One French work is referred to, and the remaining fifteen are German authorities. The paucity of our own pathological literature is so well known to the profession that it is no matter of wonder that not a single American publication figures in the list of authorities. In this respect, however, we need expect no change for the better until endowed chairs of pathology shall have been established at our hospitals and medical schools. The minute and prolonged investigations demanded by pathological investigation make it necessary that those who desire to become authorities in this branch should give their entire time to its pursuit, and there is at present with us no means by which any teacher of this fundamental branch of medicine can earn his living by it.

W. F. N.

ART. XXIX.—*The Urine and its derangements, with the application of Physiological Chemistry to the diagnosis and treatment of Constitutional as well as Local Diseases; being a course of original Lectures delivered at University College, London.* By GEORGE HARLEY, M.D., F.R.S., Fellow of the Royal College of Physicians, etc. etc. With illustrations. Philadelphia: Lindsay & Blakiston, 1872.

As is mentioned in the preface the lectures which compose this book were originally delivered to a mixed class of medical men and advanced students during a period of ten years, and afterwards published in the *Medical Times and Gazette.* They are not, however, less acceptable on this account in their present revised form, and they will well repay the student and practitioner for the time taken in their perusal.

The arrangement of the contained matter differs in some particulars from that generally adopted in works upon the Urine; and upon the whole, the method chosen by the author is calculated to render the subject more interesting to those who wish to make practical clinical use of the result of the investigations of others. In treating of each important ingredient of the Urine the author gives us first its "Chemistry," or its prominent chemical features, and the means by which it may be recognized and its amount determined; then its "Physiology," if it has one, or those circumstances attending its production in health; and finally its "Pathology," or an account of the various pathological con-

ditions accompanying its appearance or causing it, and he includes under this last head a short *résumé* of what he considers the best and most scientific mode of treatment for these conditions.

One great advantage possessed by the book is its freedom from those chemical technicalities which render so many works uninteresting to those who have not had special opportunities for practical manipulation. At the same time the necessary processes are sufficiently described for all general purposes, it not being within the intention of the author to give such further information as may be readily obtained from works upon pathological and physiological chemistry. He, however, as indeed he has shown by example, considers such extension of knowledge in the highest degree desirable for those who wish to aid in placing Medicine nearer the line of Sciences, nor does he believe that the medical chemist should give his entire attention to the study of *Pathological* Urine. He says:

"One must not, however, be led away with the idea that the urine is the royal road to knowledge, or he will be doomed to disappointment; nor must we flatter ourselves that its study is an easy one requiring neither previous preparation nor subsequent application; for, on the contrary, in order to make Urology bear fruit we require to employ in its cultivation both perseverance and skill. It is not a little remarkable that this secretion, which inspires most of us with a feeling of aversion, should not only have rendered incalculable service to Clinical Medicine, and yielded brilliant discoveries to Chemistry, but also solved many of the most abstruse problems of Physiology. Notwithstanding all this, it has not done nearly as much as it might have done, nor as it ought to have done; but this is owing to no defect on its side, but in consequence of its votaries having been always too eager to profit by its pathology, ere they had mastered the rudiments of its physiology."

To the lectures upon the more important substances found in normal urine are added two, on Albuminuria and Diabetes, which have been reproduced elsewhere, and which add to the completeness of the collection in its present form. We are surprised, however, to find no mention of the bile acids among the otherwise quite complete list of the substances found but rarely in the urine.

On the whole, we have here a valuable addition to the library of the practising physician; not only for the information which it contains, but also for the suggestive way in which many of the subjects are treated, as well as for the fact that it contains the ideas of one who thoroughly believes in the future capabilities of Therapeutics based on Physiological facts, and in the important service to be rendered by Chemistry to Physiological investigation.

H. B. H.

ART. XXX.—*Practical Lessons in the Nature and Treatment of the Contagious Diseases: an Account of the Primary Syphilitic Poison and of its Communicability.* With an appendix on the recent report of the Royal Commission on the Contagious Diseases Act, and its application to the Voluntary Hospital System. By JOHN MORGAN, M.D., F.R.C.S., Surgeon to Mercer's Hospital, and to the Westmoreland Lock Hospital, Dublin, etc. etc. Small 8vo. pp. viii., 338. London: Baillière, Tindall, & Cox, 1872.

WE have not been very favourably impressed by either the matter of Dr. Morgan's book, or the manner in which it is presented. The author is what is called a "unitist" or "unicist" in his doctrines: that is to say, he does not believe in the essential diversity of chancroid and syphilis; but he seems to enter-

tain somewhat confused notions as to what the views of "dualists" really are. His observations and experiments do not bring to his readers' minds that degree of conviction which they evidently do to his own; for the patients whose cases he describes seem to have been in most instances prostitutes (who, without any play upon words, must be acknowledged not to have furnished that virgin field for inquiry which is so essential in an investigation of this nature), and in every instance he appears to have relied implicitly upon the accuracy of the accounts which his patients gave of their previous history. In one respect we think Dr. Morgan's book will serve a good purpose; and that is in calling attention to the possibility of infection by vaginal discharges in women who are syphilitic, but in whom the disease is in a state of quiescence.

The author's style is by no means lucid, and we observe several verbal mistakes, some of which are so frequently repeated that they can scarcely be ascribed to mere carelessness: such is the constant use of the word *osteoscopic* instead of *osteocopic*. The book, as a whole, is poorly set forth, and the illustrations, with a very few exceptions, are utterly beneath criticism. We must not omit to mention that Dr. Morgan expresses himself quite favourably as to the practice of syphilization, and in order to promote the efficiency of lock hospitals, recommends that they should be provided with special attractions in the matter of diet and accommodation, so as to induce patients to enter them more readily, and to be willing to remain in them until cured. J. A., JR.

ART. XXXI.—*The Treatment of Venereal Diseases: a Monograph on the Method pursued in the Vienna Hospital, under the Direction of Prof. Von Sigmund, including all the Formulæ.* By M. H. HENRY, M.D., Surgeon to the New York Dispensary—Department of Venereal and Skin Diseases, etc. Adapted and arranged from the German. 8vo. pp. 49. New York: William Wood & Co., 1872.

IN this slender volume, Dr. Henry, the accomplished editor of the *American Journal of Syphilography and Dermatology*, has given to his professional brethren a valuable practical contribution to the literature of a most important subject, and one concerning which, in spite of all that has been written upon it, a good deal of uncertainty, not to say ignorance, still prevails. As the author justly remarks, in his introductory paragraph, the assumption that every medical practitioner is perfectly familiar with the therapeutics and essentials of treatment in venereal disorders, is proved by experience to be ill-founded: there is, on the contrary, we believe, no branch of practice in which intelligent and otherwise skilful surgeons are more apt to err, than in that which is concerned with the management of this particular class of affections. This is readily accounted for by the fact that there is afforded in this country little or no opportunity for the clinical study of venereal diseases, and by the fact that works on general surgery—to which the majority of practitioners of course look for information on these topics—necessarily can deal only with the broad principles and outlines of treatment, and are for want of space forced to ignore the multiple details which, though taken separately they may seem of trifling importance, yet in the aggregate combine to render the treatment of any individual case successful or unsuccessful according as they are observed or neglected.

It is as a contribution towards filling this acknowledged gap in the means of

education accessible to American practitioners, that Dr. Henry's monograph is offered; and affording, as it does, a concise but comprehensive view of the minutiæ of practice adopted in the Vienna Hospital (the Venereal Department of which is at least unsurpassed in the excellence of its appointments), it forms a contribution of no little importance. To study an accurate record of the details of clinical practice is the next best thing to being able to watch that clinical practice in person; and we cordially commend to our readers' consideration the practical rules for treatment which are here given, and the rich storehouse of formulæ which have been found serviceable in Prof. Von Sigmund's large experience. J. A., JR.

ART. XXXII.—*Essay on Vaccinal Syphilis.* By G. LANOIX, M.D., Member of the Legion of Honour; Chief of the "Service des Vaccinations" of the Hospitals of Paris, 1865–1870, etc. etc. 8vo., pp. 8. Published for the Author: New York, 1872.

EVER since the days of Jenner there have been medical writers, some even of note, who have attributed to the process of vaccination, when performed with virus procured from the arm of an individual who is labouring at the time under some infectious blood-disease, the power of communicating the same to the vaccinated subject. This view of the danger incurred in vaccination, having reached the public ear, and supposed to be derived from accurate and repeated observation, has been, and still remains, a formidable obstacle to the prompt and general submission to vaccination of the children of all portions of the community.

To prove the communicability of syphilis by the use, in vaccinating, of matter from infected subjects, is the object of Dr. L. in the essay before us. He describes the manner of the propagation of syphilis by vaccination as twofold: 1st. By rousing into activity the latent syphilitic poison, existing in the blood of the vaccinated individual, named by Dr. L. "*Envolved Vaccinal Syphilis;*" and 2d. By the direct conveyance into the system of the patient, of syphilitic in conjunction with the vaccine virus, which Dr. L. names "*Inoculated Vaccinal Syphilis.*"

The evidence adduced in support of these views is very far from being conclusive. We must say, positively, that we have never met with a solitary case, during a very extensive practice of over half a century; during thirty-two years of which we held a public appointment, the duties of which necessarily demanded a close attention on our part to everything connected with the pathology of the vaccine disease, and to every cause or occurrence interfering with its prophylactic power as to variolous infection; with not a solitary case, we repeat, which caused us to suspect that by the process of vaccination any danger was incurred of communicating, or of evolving, in the vaccinated, any form of syphilitic disease. We have never, in a single instance, observed the series of phenomena laid down by Dr. L. as diagnostic of either of the forms he describes of "vaccinal syphilis—the evoked or inoculated." It is true, in our vaccinations and revaccinations, we have been careful that the matter we made use of should be genuine, pure, and from the arm, so far as could be ascertained, of a healthy child.

If the insertion of vaccine matter into the person of one who is affected with latent syphilis, is capable of evoking the latent poison into action, we should cer-

tainly have seen this occasionally exemplified in reference to the pupils of our public schools, some hundreds of whom were subjected to vaccination at the instance of the College of Physicians of Philadelphia, by a committee of three of its Fellows, of which we were a member. Some of these children remained under our observation permanently for twenty-five years subsequently.

Dr. L. remarks, in conclusion, that from the danger incurred by vaccination as set forth by him, he knows of no "certain and practical" means of escape. "Animal vaccination" he believes to be "the only resource which gives us a logical and practical solution to the problem so earnestly sought after."

It is unnecessary for us to enter upon the question of animal vaccination. Late experience in France, Belgium, etc., does not appear to be in its favour. To resort invariably for vaccine matter to the cow or horse would be a preposterous suggestion, which, even if it could be carried out, would be productive in many cases of unfavourable results. It is certainly an unnecessary procedure. Proper care in the selection and preservation of vaccine matter, its proper insertion in the arm, and care that its regular course throughout its stages of incubation and maturation is not disturbed are all the precautions required in order to render the vaccine infection complete, and unaccompanied by danger of any kind.

There is, moreover, a commercial feature evident in Dr. Lanoix's publication, which is calculated to excite distrust as to the disinterestedness and reliability of the doctrines he endeavours to inculcate. D. F. C.

ART. XXXIII.—*The Question of Quarantine. The Nature and Prevention of Communicable Zymotic Diseases. A Paper read before the Medical Library and Journal Association of New York, January 5th*, 1872. By ALFRED L. CARROLL, M.D., etc. etc. 8vo. pp. 22.

THIS is a highly interesting pamphlet on a most important subject. Is it possible to prevent the introduction of any disease by the quarantine of infected vessels, their crews, passengers, and cargoes, or of infected persons and merchandise, by a strict *cordon sanitaire* embracing mainland routes of intercourse and trade? and, if so, what are these diseases? where do they originate? What, also, are the quarantine restrictions and rules demanded for their exclusion, with the least inconvenience to those who are their subjects, and the least possible obstruction to commerce and trade and to intercourse of individuals? All these points are directly involved in "the question of quarantine," and are briefly, but candidly, discussed by Dr. Carroll. His general conclusions are as follows:—

"I. Communicable zymotic diseases depend upon material organic poisons, and although some of them (as plague, cholera, etc.) may appear to be endemic in certain localities, it is probable that they exist there only by the retention and recrudence of their specific contagion; it is almost certain, at all events, that they do not arise spontaneously elsewhere.

"II. 'Quarantine of observation' should in all instances apply to living beings in whom contagion may remain latent, rather than to inanimate substances, which may be disinfected at once.

"III. Preventive measures should be adapted to the respective modes of contagion of the several diseases; stricter isolation being necessary in the case of those poisons which are volatile enough to be conveyed in the air, or in vapour of water, than with those which are transmissible only by solid or liquid media.'

D. F. C.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *Variation in the size of the Red Corpuscles of the Blood as the result of various Reagents.*—The results of 40,000 measurements in 174 animals of different species have shown M. Manassein that the size of the red blood-corpuscles *diminishes* under the action of septicæmic poisoning, exposure to a high temperature, and to an atmosphere containing an excess of carbonic acid, whilst they *enlarge* under the influence of oxygen, and agents lowering the temperature of the body, as cold, quinia, cyanic acid, and alcohol. Morphia, however, forms an exception. The corpuscles enlarge in acute anæmia.—*Brit. & For. Med.-Chir. Rev.*, April, 1872, from *Centralblatt. für die Med. Wissensch.* No. 53, 1871.

2. *Nerves of Taste.*—Dr. Ph. Lussana has furnished (*Archives de Physiologie*, March and May, 1872) an interesting addition to our knowledge of the nerves of taste derived from pathological observations. He relates the case of a woman, in whom Dr. Vanzetti twice excised a portion of the lingual nerve; two years afterwards it was found that the tongue was normal in form, nutrition, and colour on the affected side, but had entirely lost its gustatory sensibility, sweets and bitters being alike unperceived. This result he points out might have been anticipated by our knowledge of anatomy, since it is known that the lingual alone ramifies in the mucous membrane of the anterior part of the tongue, to the exclusion of the glosso-pharyngeal, which is distributed to the posterior region of the lingual mucous membrane alone. He next proceeds to show that the gustatory fibres of the lingual nerve *do not come from the fifth*, and this he proves by adducing cases of complete paralysis of the fifth, yet in which taste was perfectly preserved. They come from the seventh, as is shown by the loss of taste when the facial was divided or paralyzed, and are, in fact, contained in the chorda tympani.

3. *Excretion of Phosphates by the Kidneys.*—Prof. Falck. of Marburg, relates a series of experiments made by him, the object of which was to determine in what proportion and in what time various quantities of phospates of soda introduced directly into the blood are eliminated through the kidneys. The details of three experiments on dogs are given. In the first of these, 5.395 grms. (83 grains) were introduced, and it was found that in seven hours the whole quantity had passed into the urine. The elimination occupied the whole of this period, the largest amount being in the second hour after the in-

troduction. In the other two experiments the quantities were 10 and 9.5 grms. respectively (154 and 146 grains), and it was found that these quantities were not wholly eliminated by the kidneys, a part probably passing out of the body by the intestinal canal. So that it would appear that the kidneys have only the power of excreting phosphates up to a certain maximum rate; that, therefore, when large quantities are present in the blood, their excretion is very gradually accomplished, and when the quantities are very excessive, the intestinal canal is brought in to aid the kidneys.—*Glasgow Med. Journ.*, May, 1872, from *Virchow's Archiv*, December, 1871.

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4. *Urea in the Animal Body*,—GSCHEIDLEN has examined almost every tissue in the body for urea, and finds it universally distributed, except in the muscles, in which it is never present.

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5. *Investigations on Low Organisms.*—Prof. RINDFLEISCH, of Bonn, has published (*Virchow's Archiv*, December, 1871) some investigations on this subject which are of great interest, and which, when further prosecuted, are likely to lead to important results. His object has been to find a method by which he could cultivate fungi, in such a manner as that, while the substance is exposed to the air, it should be protected from all impurities. With this view he prepares a square microscopic cover-glass, by placing a small drop of wax at each corner, so that when the cover-glass is placed on the slide it is supported as on four pillars of hard wax. The substance to be observed is placed in the centre of a well-cleaned glass slide, and the cover-glass is then placed on it, being afterwards slightly fixed by wax at the four corners. By this means the substance to be investigated is situated beneath the centre of the cover-glass, and around it, under the cover-glass, is a thin zone of air. But this zone is so thin that it will be perfectly still, and no dust will be carried in by currents, while at the same time there is nothing to interfere with any chemical change which may occur between the air and the growing fungus. This method being devised, the author proposes to sow various substances with the spores of different fungi, and observe the results. It is to be observed that during the intervals of observation the preparations are kept in a moist atmosphere. The first observations recorded were made with mould fungus. A portion of apple pulp was placed on a slide, and this sown with spores of the fungus found in rotten apples (*botrytis cinerea*) and the growth of the fungus was seen to take place in the most beautiful manner. It was remarked that, strangely enough, though the mycelium of the fungus grew continuously, the spores were only produced at night. The favus fungus (*achorion Schœnleinii*) was next subjected to observation. A favus crust was moistened with a drop of distilled water, and treated as above. The fungus grew, but its growth was comparatively slow, threads growing out from the moistened crust and producing spores. The spores were rod-shaped as they were formed, but on the addition of a drop of water they swelled up and became globular. The same fungus was also cultivated on a portion of the fruit from plum jam, and the result was a much more vigorous growth than when the favus crust was merely moistened. The different parts were distinctly larger and the growth richer, so that the author describes it as quite a tropical growth of the fungus. We are glad that the author promises still further observations of this interesting nature.—*Glasgow Med. Journ.*, May, 1872.

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6. *On the Occurrence of Fungi in the Blood of healthy Men.*—Dr. LOSTORFER has published some observations made by him on blood taken from the finger of healthy men, with precautions to insure against contamination. A single drop was placed on a slide and preserved from evaporation by being kept in a moist chamber. During the first two days nothing noteworthy was remarked; but on the third day there appeared pale globular granules grouped together to the number of two or four, up to twelve. On the fourth day these groups had enlarged both by increase in size in the individual granules, and increase in the number of granules forming the groups. At the same time the granules had somewhat changed their shape, becoming from globular more

angular, so as to resemble the sarcina-form. On the fifth day the groups were still larger, this depending very little on increase in size of the granules, but chiefly on increase in number. As they resemble the sarcina in form, so do they also in their mode of increase as actually observed, this mode of increase being by division. They differ from the commonly known appearances of sarcina in certain points; (1) they are much smaller; (2) the groups are made up of a much larger number of individual granules; (3) they are always colourless, whereas the sarcina ventriculi has generally a yellow tinge. But various observers have already shown that the same fungus may exist in somewhat different conditions under varied circumstances (see even the case of the favus fungus in the article from *Virchow's Archiv*, abstracted above), and the author has found that when Pasteur's fluid is added to the blood in which these organisms have become developed, they become considerably larger, their angles become more marked, and they take on a yellowish tinge, so as to approach much more nearly to the better known appearances of sarcina. The author himself has no doubt as to their identity with the sarcina ventriculi. These organisms are not present in the blood while in the vessels, but these experiments seem to show, that in the majority of cases at least, their germs are there present, and that when the blood is effused and left at rest, the sarcinæ may develop. It is to be remarked that the development of the sarcinæ was not a phenomenon of the ordinary process of decomposition of the blood; the experiments were made with special precautions against the admission of germs of bacteria, etc., and as an actual fact the sarcinæ appeared before any signs of decomposition, or the development of bacteria, so that the author seems warranted in concluding that their germs were present in the blood at the time of removal. The author then refers to various instances in which the occurrence of sarcinæ in other parts of the body than the stomach has been recorded. It has been observed in the lungs, and in the urine especially, and he has no doubt that in these cases the blood was the source from which the germs of the organism were derived. There is also a very strong presumption that even in the stomach the germs are supplied by the blood.

The very remarkable fact of the development of sarcina in healthy blood has been confirmed by Dr. Ferrier, whose investigations are published in the *Brit. Med. Journ.*, January 22, 1872.—*Glasgow Med. Journ.*, May, 1872, from *Stricker's Jahrbücher*, Pt. IV., 1871.

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7. *Precocious Development.*—M. Flugel (*Bagr. Arztl. Int, Blt.*, No. 49, 1871) gives the account of a female child who had died at the age of 5 years and 6 months, from an attack of dysentery, and at that period had attained the height of 5 feet, and a proportionate development of the body throughout. When *six* months old she had cut all the incisor teeth, when nine months old all the molars. When she had reached her eighteenth month, her catamenial discharges first made their appearance, and from that time occurred with great regularity, especially during her later years. Whilst present she exhibited an unusual degree of liveliness. The hair of her head was long and beautiful. The mamma prominent, external genitals well developed but devoid of hair. The pelvis was capacious. The condition of the internal sexual organs was not ascertained. The intellectual powers of the child, although for six months preceding her death she had begun to prattle, did not appear to be any more advanced than customary at her age.—*Centralblatt f. d. Medicinish Wissenschaften*, Feb. 17, 1872. D. F. C.

MATERIA MEDICA AND GENERAL THERAPEUTICS AND PHARMACY.

8. *Action of Chloral Hydrate.*—The author of a prize essay, presented to the Medical Society of Lyons, has observed (*Lyon Medical*, 1872, p. 296) that the action of chloral on animals varies according to stature, age, and race; old dogs resisting its action less than young or adult ones, and hounds and spaniels

being more sensitive than bull-dogs and terriers. It acts most quickly when injected into the veins, less quickly when given by the mouth, and more slowly still when injected subcutaneously; four or five *grammes* dissolved in fifteen or twenty *grammes* of water acting in ten to fifteen minutes when given by the mouth, in fifteen to twenty when injected subcutaneously, and almost immediately when introduced into a vein, when it may prove fatal. When injected subcutaneously it causes inflammation, which may proceed to sloughing; when given by the mouth its acrid taste may induce vomiting, and it always increases the secretion of saliva. Its first effect is on the spinal cord, impairing movement, so that the walk of the animal becomes irregular and stumbling, and it finally falls and its muscles become flaccid. Its second action is on the brain, the sensibility of the animal becoming first impaired and then entirely abolished. At first the animal feels pricks and burns, moving and growling when hurt, but afterwards it becomes completely insensible, and then its life is in danger. Thirdly, it paralyzes the sympathetic, producing contraction of the pupils, and congestion of the skin and mucous membranes, mesentery, and brain. As the action increases, the cardiac ganglia become paralyzed, the heart beats slowly, the respiration diminishes, and the temperature falls. The sequence of the symptoms is the same in man, with the exception that the conjunctiva becomes first anæsthetized and the nasal mucous membrane last. Chloral, when mixed with blood and heated to 40 deg. C. (104° Fahr.), is decomposed and yields chloroform. The blood of a chloralized animal warmed to the same temperature also gives the reaction of chloroform, and the greater the alkalinity of the blood the more quickly does it do so. The whole of the chloral is decomposed into chloroform and formic acid by the blood, for, after it has been mixed with blood and warmed, the indications of the presence of chloroform are not increased by the addition of sodium carbonate to the blood. The chloroform and formic acid are finally converted into sodium chloride and formiate, and eliminated by the urine. The author believes the differences between the action of chloral and chloroform to be due to the chloroform given off from chloral being in a nascent condition. In cases of suppurative arthritis, white swelling, or necrosis, where chloral given by the mouth, as well as subcutaneous injection of opium, have not afforded relief, the author has seen much benefit derived from the local application of chloral either to a sinus or endermically. He finds it useful in the cough of measles and in whooping-cough. It is of little use in epilepsy. It is best given in syrup of tolu or syrup of oranges. The ordinary dose for an adult is two or three *grammes*; for children under five years, not more than fifteen to twenty *centigrammes*; under ten years, twenty-five to fifty *centigrammes*; between ten and fifteen years, one to two *grammes*. Hypodermic injections of one *gramme* of chloral in one *gramme* of water are useful in furious mania, delirium tremens, and tetanus. This method of application has the inconvenience of producing local inflammation. Chloral may be employed endermically by placing one *gramme* of it in powder on a wound or fistulous opening, or on a blistered surface about an inch in diameter. It produces a soft grayish slough, which falls off in two or three days, leaving a wound ready to receive a fresh dose. The wound should be covered with lint or charpie to prevent the dissolved chloral from running over the adjacent skin and blistering it. Chloral is contraindicated by gastric irritation, by organic lesions of the intestinal canal, and by laryngitis. In large doses, it will be hurtful if cardiac or cerebral disease be present. The author finds from four experiments that the action of chloral and strychnia are antagonistic, but the action of strychnia is more powerful, so that an animal put asleep by chloral can be awakened by strychnia, but one under the action of strychnia cannot be put to sleep by chloral. The action of Calabar bean and chloral are antagonistic, but that of chloral is more powerful than that of the bean.—*Brit. Med. Journ.*, May 11, 1872.

9. *Action of Digitaline on the Circulation and Temperature.*—ACKERMANN (*Berlin Klin. Wochenschr.*, 1872. p. 27) thinks that digitaline renders the pulse slow by acting on the ends of the vagus in the heart. This action is prevented by atropia. Large doses quicken the pulse, both by paralyzing

the vagus and stimulating the musculo-motor apparatus or quickening nerves of the heart. This quickening is frequently followed by slowness and irregularity, which are probably due to commencing paralysis of the muscular substance of the heart. The stoppage of the heart which succeeds this, or which may even occur immediately after the primary retardation, is independent of any action on the inhibitory nerves of the heart, and is due to paralysis of musculo-motor apparatus. The blood-pressure is increased by the injection of digitaline. This rise in pressure is independent of the pulse-rate, and is chiefly or entirely due to irritation of the vaso-motor nerves. It is less when the spinal cord is divided at the atlas previous to the injection. It is sometimes preceded by a fall, due to the digitaline exciting the vagi before the vaso-motor nerves. The rise of blood-pressure is followed by a fall, which is chiefly due to vaso-motor paralysis, but partly also to cardiac paralysis. The undulations in blood-pressure first noticed by Traube, and discovered by Haidenheim to depend on the relations of the respiration to the activity of the heart, are rendered slow by digitaline, but in exactly the same proportion as the pulse, so that the number of pulse-beats in each undulation remains unaltered. In previous researches, he had found no alteration in temperature produced by digitaline in the temperature of the body. This he had measured in the rectum, which, as Haidenheim has shown, is unsuitable for such observations. On measuring it in the vena cava with one of Haidenheim's thermometers, he found that the temperature fell when the blood-pressure rose, and rose when blood-pressure sank. The temperature rose over the normal when the blood-pressure fell below it. The fall of temperature which accompanied the rise in blood-pressure was most marked when the pulse was frequent and small, and the pressure less, before the injection of digitaline; and was less when the pulse was quick, even although the pressure rose. The benefit produced by digitalis in dropsies is due, Ackermann thinks, to the excitement of the vaso-motor nerves, which increases the pressure of blood in the arteries, lessens it in the veins, and quickens the circulation. These conditions cause absorption of fluid from the tissues; and the fluid thus introduced into the blood, along with the increase in the arterial pressure, produces diuresis. The antipyretic action of digitaline may be due to the fall of temperature which accompanies the increase in blood-pressure.—*Brit. Med. Journ.*, May 11, 1872.

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10. *Action of Antimony.*—From a series of experiments on frogs and other animals, Dr. S. RADZIEJEWSKI comes to the following conclusions in regard to the action of antimony: 1. The emetic effect of the tartrate of antimony introduced into the stomach is the result of the action of the drug on the mucous membrane of the stomach, and not of its absorption into the blood. The effect of the peculiar irritation of the stomach produced by the substance is, by reflex action, to produce vomiting. 2. The depression of the action of the heart is also a result of reflex action, and not, as some have supposed, a special result of the action of the potassium contained in the tartrate. 3. Antimony has, in addition to its effects on the intestinal canal also a general action on the organism corresponding to that of arsenic.—*Glasgow Med. Journ.*, May, 1872, from *Reichert and Du Bois-Reymond's Archiv*, Part IV., 1871.

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11. *Physiological Action of Tobacco-smoke.*—VOHL and EULENBERG find that the amount of nicotine in snuff and tobacco for chewing is so minute that nothing like nicotine-poisoning can result from their use. The action of tobacco-smoke and tobacco-juice is not due to nicotine, for it contains none, but to pyridine, picoline, colledine, and other bases forming a homologous series, which are produced during the combustion of tobacco. The reason why stronger tobacco can be smoked in a cigar than in a pipe is, that in the pipe a large quantity of pyridine is formed, which is very volatile and stupefying; while in a cigar little pyridine and much colledine are formed. The unpleasant symptoms experienced by persons just beginning to smoke, or who smoke in excess, as well as the poisonous effects of tobacco-juice, are not due to nicotine, but to the pyridine and picoline bases; and they have probably been attributed to nicotine because these bases, especially those having a high boiling-point,

greatly resemble nicotine both in smell and in physiological action, producing contraction of the pupil, difficult respiration, convulsions, and death. They do not act so quickly when injected under the skin as when taken into the stomach, and their action is less rapid than that of nicotine. Other plants which are sometimes used for smoking, though they contain no nicotine, such as dandelion, willow-wood, and stramonium, yield pyridine bases whose action is very like those from tobacco, though rather weaker. Pure picoline from Boghead coal had a similar action; and its vapour was poisonous, producing irritation of the respiratory passages, slight convulsions, and death. None of these, except the bases from willow-wood, produced contraction of the pupil. The authors consider that the effects produced by opium-smoking are not due to the alkaloids in the opium, but to the bases formed during its combustion; and that the difference between it and tobacco is owing to different bases being formed by their combustion.—*Brit. Med. Jour.*, May 11, 1872, from *Arch. Pharm. Chem.*, cxlvii. p. 150.

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12. *Carbolic Acid and its Therapeutic Properties.*—Dr. ERNEST LABBÉE, in a very elaborate paper on this article, after alluding to its chemical characters, refers to the fact that it is a violent poison to all living beings, and is especially remarkable for its toxic action on microscopic forms of life, both animal and vegetable; and hence, as is well known, it has been extensively employed, on theoretical grounds, in the treatment of accidents or injuries or diseases, the peculiarities or complications of which are supposed to be influenced by the presence of minute living organisms. Dr. Labbée treats at length of the experiments of modern observers, who are said to have traced living germs in the products of fermentation, and even in the miasms from the human body, but he neither denies nor confirms their statements, and in the same spirit he refers to the views entertained and published as to the connection between the phenomena of disease and the development of the morbific germs. Carbolic acid has also been strongly recommended as a disinfectant, its efficacy in this respect being supposed to be due to the power it has of destroying the life of the minute organisms which are assumed to be the causes of epidemic and infectious diseases; but in this respect, also, Dr. Labbée seems to think that the theory on which its use has been founded is not well established, while in practice its efficacy is still doubtful. The physiological effects of carbolic acid are local and general; when applied to the skin or the mucous membranes, it acts as a caustic; when given internally, the effects vary with the dose, but when this is large the acid acts as a violent poison. Among other effects which it produces, there is a notable diminution in the temperature of the body; and with respect to its elimination from the system, this process seems to take place from the lungs, and perhaps also from the skin. Dr. Labbée relates some experiments made by himself on frogs with carbolic acid, and the general results were that it appeared to act on the nervous system, causing convulsions, which were rather epileptiform than tetanic, and its special influence seemed to be upon the cerebellum and the medulla oblongata. In reference to the employment of carbolic acid in surgery and medicine, the author, while copiously quoting the experience of English and French authorities, expresses his own opinion that in surgery it is not superior in efficacy to other remedial agents, such as alcohol, glycerine, etc., and that it has no decided influence in preventing complications in the healing of wounds. At Sedan, where Dr. Labbée made use of the acid very extensively, he did not find that it prevented death from purulent infection after amputation, or that it neutralized the fetor of the wounds. In comminuted and compound fractures, however, he found that dressings of carbolic acid were very useful. In medical practice, Dr. Labbée passes in review the various applications of the acid in various forms of disease, and while he does not confirm the reports as to its efficacy in intermittents, typhus, or the eruptive fevers, he considers it very useful in many kinds of skin diseases; and from its slightly caustic and astringent properties, he thinks it may be useful when given in some forms of atonic dyspepsia. He also thinks that from its power of reducing the animal temperature it might be given advantageously in febrile conditions attended with excessive calorification. On the

whole, Dr. Labbée arrives at the conclusion that, in spite of many illusions in regard to the acid and some exaggerated reports as to its utility, it is really an important article of the materia medica.—*Brit. and For. Med.-Chir. Rev.*, April, 1872, from *Archives Générales*, Oct. 1871.

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13. *Therapeutic Value of Myrrh.*—Dr. Delioux de Savignac states that myrrh contains in 100 parts 2.60 of volatile oil, 27.80 of resin, 63.70 of gum, and 5.90 of salts and impurities. Its physiological action is comparable on the one hand to the balsamic substances, and on the other to the fetid gum-resins. Its general stimulating properties are more active, and its local action is more lively, than those of benzoin, storax, the balsams of Tolu and Peru, and almost equal to that of tar. Like most resinous compounds, it was formerly regarded as resolvent and deobstruent, and it was prescribed for visceral engorgements. It is possible that in consequence of its stimulating properties it may render resolution more active, but as it was commonly combined with purgatives, these would play a great part in clearing the excretory passages and removing engorgements. It does not itself purge, but there is no evidence that it constipates as the resins generally do. The part of the digestive tube which Dr. de Savignac believes to be most advantageously acted on by myrrh is the stomach. His experience fully confirms the stomachic properties that have been attributed to it from remote antiquity. He has seen painful dyspepsia rapidly relieved and even cured by the use of this gum-resin, either prescribed alone or in combination with other medicines, as bismuth, bicarbonate of soda, etc. Myrrh in these cases relieves the pain, awakens appetite, gives tone to the stomach, and renders digestion more active and regular. Besides these actions, it proves a general tonic in those gastralgiæ that are connected with various chronic diseases, and particularly with anæmia and chlorosis. But this last is often accompanied by amenorrhœa, and as strength returns the catamenia reappear. Cullen regarded myrrh as being only indirectly an emmenagogue; but Sydenham and many others have thought it exerted a direct action on the uterus. Its pectoral properties, he thinks, have been exaggerated, though it may aid other remedies in calming the cough and facilitating expectoration. He has found it particularly useful in vaginal and uterine catarrhs, and especially when applied topically in the form of vaginal injections, containing one to three drachms of tincture or vinegar of myrrh in a quart of water. Mattioli and some others have prescribed it with success in intermittent fevers, but further evidence is required on this point. Alibert admits its value as a topical remedy in skin affections, and it has been largely used for ulcers, for the dressing of wounds, and cases of caries and necrosis, and as an antiseptic in wounds of the soft parts of a bad nature, likely to become gangrenous. For this purpose the tincture of myrrh is best adapted. As an internal remedy, acting as an excellent stomachic, he strongly recommends the following prescription for the wine of myrrh:—

Picked myrrh	20 grammes.
Rind of Seville oranges . .	15 "
Malaga wine	1½ pint.

Macerate for ten days, and filter.—*Practitioner*, April, 1872, from *Bull. de Gén. Thérapeutique*, t. lxxxi., livr. 11.

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14. *Lacto-phosphate of Lime as an Analeptic Medicament in Adynamic Fevers and in Convalescence.*—Dr. R. Blacke extols (*The Practitioner*, February, 1872) the efficacy of the lacto-phosphate of lime as an analeptic in adynamic fevers and in convalescence. "It is at once," he says, "an aliment and an article of food, and a medicament of the highest value. Its administration cannot, like that of alcohol, produce mischievous consequences, and it never depresses the nervous system charged with presiding over the transformations which take place in the nature or form of the elements of our tissues."

In children who, during the process of dentition, or the various critical stages of development, and especially puberty, often present an indifference to, and sometimes a deep disgust for, all kinds of food, whence result atony of all the tissues, and often diarrhœa or constipation—in such cases the patients gene-

rally will not tolerate cod-liver oil; but the lacto-phosphate of lime, being extremely palatable, is readily accepted by all young patients, and is, Dr. B. asserts, extremely beneficial.

"In aged persons," says Dr. B., "we not unfrequently meet with an almost complete suspension of all the organic functions, and more especially those of digestion and of the various acts connected therewith. The wine of lacto-phosphate of lime, administered at the end of meals, permits digestion, increases the assimilation of alimentary substances, and awakens muscular energy, which is so often abolished at this time of life.

"This sinking of all the organic functions contributes powerfully to give to all the acute affections of the aged (and of those who resemble the aged by a condition of general exhaustion) a peculiar character of adynamia. In these cases, a febrile state excited by any cause brings on fatally such a movement of denutrition that the patient often dies, notwithstanding the most diligent and appropriate treatment. If, as I believe, it is by acting on the ganglionic nervous system, and by suspending to a certain extent a powerful process of disassimilation, that alcohol has rendered such great services and been so very successful in cases of pneumonia and other acute affections marked by an adynamic character, does it not seem more rational to leave untouched the functions of the nervous system, and seek merely to fulfil an indication so tritely put down by one of England's great clinicians?—feed the fever. And if this position be correct, I think I may safely assert that the lacto-phosphate of lime is, of all substances, the one best calculated to act as an aliment and as an excitant of the nutritive functions, in the numerous cases to which it applies."

In another class of diseases of an adynamic character, viz., typhoid fever and typhus, this remedy, Dr. B. states, has justified his most sanguine expectations. In the last stage of typhoid fever, when the acute phenomena have disappeared, "we are often," says Dr. B., "placed between two alternatives: either the organism, exhausted by its recent struggle for life, remains prostrate, and allows only of an insufficient alimentation, or, the appetite being awakened, the too small quantity of gastric juice does not allow the stomach, still much enfeebled, to digest the food which the patient takes.

"Now, in these cases we find in the lacto-phosphate of lime at once the chemical agent of digestion and the natural excitant of nutrition, which permits us to overcome two formidable evils, and to secure a speedy restoration to the normal condition."

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15. *Antipyretic Action of Quinia and Alcohol.*—BINZ (*Virchow's Archiv*, li.) is inclined to attribute the action of quinia more to its direct effect on the animal fluids than to its nervine action. Some experiments by Naunyn and Quincke, however, appear to have given ground for the idea that it depresses the temperature through the agency of the nervous system. They observed, when the temperature rose after pinching the spinal cord in dogs, that the elevation of temperature could often be counteracted by large doses of quinia. Binz has repeated these experiments with the above-named experimenters, and has observed distinctly in two cases this result. It was observed, in regard to the animals, that, when experimented on, there was great *post-mortem* elevation of temperature and very rapid putrefaction. In those which received quinia these results were almost completely prevented. The negative results often obtained in regard to quinia are, in some cases, to be attributed to the rapid conversion of the substance into the inactive dihydroxylquinia, and in other cases to the fact of its being administered in an insoluble form. Quinia ought to be given in solution. The best form is the hydrochlorate. The above experiments do not yet decide the question whether the action of quinia is dependent upon the nervous system or not. Alcohol seemed to give more precise results. Dogs treated as in the experiments of Naunyn and Quincke received, within an hour and a half or three hours after the operation, about sixty cubic *centimetres* of alcohol diluted with water. In most of the cases, there was a very distinct inhibition of the temperature more marked than with quinia. The *post-mortem* elevation of temperature was almost inappreciable. Alcohol is very rapidly used up in the febrile condition. The action of alcohol

Binz attributes to direct action on the tissue-metamorphosis. A number of facts point to alcohol as being an inhibitor of oxidation in the tissues. From these effects of alcohol, Binz thinks that quinia is more likely a humoral than a nervine agent.—*Brit. Med. Journ.*, May 11, 1872.

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16. *Eucalyptus Globulus as an Antiperiodic.*—Strong evidence has been adduced to prove that the eucalyptus globulus, a native of Tasmania, but now largely cultivated in the south of France, Spain, Algiers, etc., possesses very decided antiperiodic properties. Two years since Dr. Lorinser, of Vienna, published the results of his observations on the treatment of intermittent fever by the tincture of the leaves of this tree, and Bohn was equally successful with it. (See No. of this Journal for April last, p. 518.)

More recently Dr. Joseph Keeler, chief physician of the Austrian Railway Company, has treated with the tincture of eucalyptus 432 patients. (*Brit. Med. Jour.*, May 11, 1871.) Of these, 310 (71.76 per cent.) were perfectly cured, and 122 (28.24) required to be afterwards treated with quinia. Of the 310 patients who were cured, no paroxysm occurred after the first dose in 202; in the remaining 108, there were one or more subsequent paroxysms, which, however, yielded to repeated doses of the medicine. Quinia had been given without result in 118 of the 432 cases; 293 of the patients had had ague in previous years, and 139 were attacked for the first time in 1871. Of the 122 cases in which the eucalyptus failed, 58 recovered under the use of quinia, 38 were not cured, 10 were sent home, and 16 remained under treatment. Of the 118 cases in which quinia had been given unsuccessfully, 91 recovered under the use of eucalyptus, and in 27 no result followed.

The several types of intermittent fever were represented as follows: quotidian, complicated, 117, simple 73=190; tertian, complicated, 126, simple 95=221; quartan, complicated 16, simple 4=20; quintan, complicated, 1. The complications were, enlargement of the spleen or liver, anæmia or chronic gastric catarrh, paludal cachexia, etc. The remedy was successful in 161 of the 260 complicated cases, or 61.9 per cent.; and in 149 (or 86.6 per cent.) of the 172 simple cases. The percentages of success in the various types were: in tertian, 75.57; in quartan, 70; in quotidian, 67.89. Among the cases in which the first dose of eucalyptus arrested the disease, were 95 complicated and 107 simple; 28 of the former and 20 of the latter had previously been treated unsuccessfully with quinia. In the cases where the paroxysms recurred, there were 70 complicated and 58 simple; quinia had been given without success in 27 of the former and in 15 of the latter.

Of the 432 patients, 353 were men, 46 women, and 33 children. There were 155 patients who were immigrants into the localities; and in these the disease was more frequently attended with complications, and the treatment was less successful, than among the indigenous inhabitants.

The treatment was generally commenced on the fifth day after the first paroxysm of ague; its duration averaged 9½ days, that with quinia in previous years having been 12½ days.

The tincture was made by dividing into small pieces the leaves of eucalyptus obtained through France from the native country of the plant, and macerating in alcohol for three months. Ten pounds of the leaves yielded twenty-five quarts of the tincture. The average dose was two drachms; and the average quantity used for each patient was seven drachms—this, however, varied much, according to the nature of the case and its complications.

Dr. Keller concludes that eucalyptus must be regarded as a very important remedy for ague; but that the plant as cultivated in Austria is less efficacious, than that imported from its native soil; that the remedy is of service especially in obstinate cases of ague where quinia has been given unsuccessfully; and that the average duration of treatment by eucalyptus is shorter than that by quinia. He believes that the tincture is the most eligible preparation of the plant, as the essential oil is retained. The cost of a quart of the tincture he calculates to be less than two florins. It has a pleasant aromatic flavour. For women and children, some simple or orange syrup may be added. In the milder cases, two or three teaspoonfuls, taken before the expected paroxysm, are generally suffi-

cient. Where cachexia is present, small doses should be taken night and morning for some time.

17. *Picrate of Quinia.*—Dr. MASSE communicated to the Academy of Medicine of Paris the results of his trials with this preparation in intermittent and remittent fevers. He stated that this preparation, though it exercised a curative power in intermittent fevers, was less effectual than the sulphate of quinia. Its bitterness renders its administration difficult; and it has no curative power in bilious remittent fever. Hence it is not a true substitute for the sulphate of quinia.—*La Revue Scientifique*, May 4th, 1872.

18. *Ethyl-chloride of Mercury.*—Dr. PRUEMERS, of Berlin, states (*Virchow's Archiv*, December, 1871) that, after some experiments on animals, he used this preparation in some syphilitic cases with eruptions. He injected subcutaneously a solution containing $\frac{1}{10}$ to $\frac{1}{4}$ of a grain of the substance, and generally 10–20 injections were sufficient. The injection produced no considerable pain, and no appreciable infiltration or induration of the part, presenting in these respects a marked contrast to the injection of corrosive sublimate. There was also no disturbance of the digestive organs or salivation. He has also administered the agent in the form of pill, the dose being half a grain to a grain, night and morning. In this case also there were no unpleasant results.—*Glasgow Med. Journ.*, May, 1872.

19. *Pulvis Glycyrrhizæ Compositus, a laxative preparation of the Prussian Pharmacopœia.*—Dr. DAVID PAGE extols this preparation for the treatment of simple constipation resulting from atony of the bowels. "Whether in simple uncomplicated torpor of the intestines," he says, "or in constipation accompanying temporary gastric disorder, the powder, alone or auxiliary to appropriate remedies, is preferable to other preparations of its class. In the former, our object is rather to call into play the peristaltic action of the intestine than to deplete by serous transudation from its walls, and, in the latter especially, no prudent practitioner would run the risk of aggravating the disordered stomach by the exhibition of purgatives possessed of irritant or drastic properties. The compound liquorice powder is composed of the following constituents, so prepared as to form when incorporated an almost impalpable powder: Senna leaves, ℥vj; liquorice root, ℥vj; fennel seeds, ℥ij; sulphur, ℥iij; refined sugar, ℥xviij.[1]

"The active ingredients are sulphur and senna. The action of the former, when administered alone, is frequently accompanied by tormina, and the continued use is apt to cause derangement of the mucous membrane of the upper intestine. The physiological action of sulphur appears to be upon the muscular coat, and less upon the mucous surface, while senna is a more active purgative, more apt to excite tormina, and acts more upon the mucous than the muscular coat. By the aromatic and stimulant properties of the fennel, and the demulcent action of the liquorice, itself a mild laxative, the effects of the more active constituents are judiciously modified.

"The usual dose is a small teaspoonful at bed-time in water, with which it is easily mixable, forming an agreeable draught. Children, to whom Gregory's powder is a terror, readily take it with the belief that it is a sweetmeat.

"That the action of the powder is not to produce catharsis with serous transudation is proved by the motions, which are usually well-formed and soft."—*The Practitioner*, May, 1872.

20. *Sulphovinate of Soda as a Purgative.*—Dr. RABUTEAU has taken this salt himself and administered it to others, and finds: 1. That the sulphovinate of soda purges in relatively feeble doses; 25 grammes, or five drachms, are always sufficient; 10 grammes act on children and sometimes on adults. 2. The number of evacuations varies according to the quantity ingested; with

[1] This formula is given in the Pharmacopœia Borussica.

a dose of 20 grammes in three glasses of water, it usually produces four or five stools; and from five to eight in doses of 25 grammes. The effects usually begin to manifest themselves an hour after it has been taken. 3. The sulphovinate is the mildest of saline purgatives; it produces no exhaustion or pain, and it may even cause colics previously experienced to disappear, as for instance in certain forms of diarrhœa, which it is capable of speedily arresting. 4. This salt, causing no pain, no abnormal intestinal contraction, acting, in a word, as a type of dialytic purgatives, can be prescribed even during menstruation and pregnancy. 5. In consequence of its very slight flavour when slightly sugared, it is taken without repugnance by the most fastidious, and by children. 6. The sulphovinate of soda ought to be preferred to the citrate of magnesia, since it possesses the advantages, without the disadvantages, of the latter. In the first place, it is more agreeable to the taste, especially when dissolved in Seltzer water; secondly, it cannot lead to the formation of calculus. It is well known that it is dangerous to continue the use of the magnesian salts too long, and no judicious physician would prescribe the citrate to old people, especially to those affected with catarrh of the bladder, lest the ammoniaco-magnesian phosphate should be precipitated.—*Practitioner*, April, 1872, from *Revue de Thérap. Méd.-Chir.* 1871.

21. *A Zootrophic Powder.*—M. Polli, after dwelling on the importance of the presence of the various mineral constituents of the body in food, if the healthy nutrition of the tissues is to be maintained, recommends the following powder, as containing some of the most essential: Phosphate of lime 10 parts; tribasic phosphate of lime, 10 parts; phosphate of soda, 15 parts; carbonate of lime, 10 parts; sulphate of magnesia, 15 parts; chloride of sodium, 10 parts; bicarbonate of potash, 15 parts; oxide of iron, 10 parts; oxide of manganese, 2.5 parts; silicate of potash, 2.5 parts. The dose of this powder is 45 grains daily for children, or double this quantity for an adult. He recommends its use especially—1. In infants at the breast, who are suffering from dentition, or in nurses. 2. In children suffering from osteo-malacia, rachitis, scrofulosis, or chlorosis. 3. In pregnant women, and those suffering from puerperal cachexia. 4. In fractures, and cases of caries of the bones. 5. In tuberculosis, especially when there are vomicæ. 6. In anæmia after hemorrhages, and in leucocythæmia. 7. In convalescence after long illness.—*The Practitioner*, May, 1872, from *Medizinisch-chirurgische Rundschau*, Feb. 1872.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

22. *Experiments on Pyæmia.*—Dr. Burdon Sanderson made a most interesting and important communication to the Pathological Society of London on pyæmia. He began by relating his first experiments as to the effect of inoculating the lower animals with pyæmic liquids. In the autumn of 1867, he injected the purulent liquid contained in the ankle-joint of a patient, who had died a few hours before with metastatic abscesses, general suppurative arthritis, and intense septicæmia, under the skin in a dog and two guinea-pigs. The two guinea-pigs died within short periods (fifteen and twenty days), and exhibited symptoms of great intensity. Both had metastatic abscesses; but in one the lungs were already beset with minute nodules, resembling miliary tubercles. The dog lived seven weeks. In this case there were no secondary abscesses, but miliary tubercles of the liver and spleen. From one of the guinea-pigs, two others were inoculated, of which one died of pyæmic subcutaneous abscesses, without visceral disease; the other, which lived longer, had no abscesses, but tuberculous disease of the lungs. During the same winter, other experiments of the same kind were made, all of which seemed to show that, by the inoculation of pyæmic products, two sets of lesions might be produced: as an imme-

diate result, metastatic abscesses, accompanied by a general typhoid state, which was often fatal; as an ulterior result, either disseminated nodules, at first hard, but afterwards becoming caseous at their centres, or interstitial induration—both forms of lesion having their seat chiefly in the lungs, spleen, and liver, but also occurring in other viscera.

Having stated these facts, which he said had even in 1868 led him to regard it as probable that the two forms of infective lesions—the tuberculous and the pyæmic—were connected together etiologically and genetically, he referred to another fact which resulted from experiments made in 1871, as to the existence of bacteria in animal liquids, and the circumstances which determined their occurrence. These experiments had shown, that whereas bacteria could not be shown to be present either actually or in germ in the healthy liquids or tissues, or in the products of healthy inflammation, they were present potentially in pyæmic liquids—that is to say, that, whereas ordinary pus could be kept for days or even weeks free from bacteria, provided the precautions against "spontaneous generation" were observed, pyæmic pus could not be so kept, and, moreover, possessed the properties of at once determining the development of bacteria in any suitable liquid to which it was added. At that time he had concluded, as he now knew, from insufficient observation, that pyæmic pus did not itself contain visible bacteria.

A short account was next given of certain researches made during last summer, in association with Dr. Klein, as to the channels by which infective poisons were distributed from their centres of origin. Referring to the last occasion on which he had brought the subject of the intimate pathology of tubercle before the Society, and to the doctrine he had then advocated, that tuberculosis is an irritative overgrowth of a pre-existing tissue, he said it had then been shown that the process, in its disseminated or interstitial form, had its seat in a certain tissue, and this tissue had been termed adenoid or lymphatic—both words implying its intimate and special relation with the lymphatic system; but the precise anatomical nature of this relation had been imperfectly made out. No further progress was made till last May, when Dr. Klein came to England with the distinct object before him of co-operating in the investigation of this very question. The field taken up was the peritoneum; the reason of the choice being, that that membrane, and especially the omentum and diaphragm, had already been the subject of investigation, as favourite seats of tuberculosis. Those researches had not merely served to elucidate one or two anatomical facts of very great importance to the pathologist—*e. g.*, the existence of a lymphatic system in the omentum and its distribution, and the mode in which the peritoneum communicates with the lymphatic system—but had rendered it possible to give an account which, so far as the peritoneum was concerned, was tolerably exact and complete, both of the normal process of absorption and of the changes which the absorbing tissues undergo when they are entered by infective agents.

In the course of these experiments, it was found that not only as regarded the property of any given peritonitis to assume the infective character, but as regarded the intensity of the infective results and their duration, there were endless varieties. In one set of cases, the secondary lesions were suppurative, the constitutional disturbance intense, and the fatal result rapid; in another, the lesions were vascular new growths, firm at first, afterwards becoming caseous, the progress slow, and the functional disturbance imperceptible. And then it appeared that in all those instances in which the pyæmia—*i. e.*, the acute character—manifested itself, bacteria were present, not merely in the purulent liquids, but in the blood. Under these circumstances, attention was directed from the effects to the poison itself. Soon after the opening of the Brown Institution, it was found that the practice of the hospital for animals was likely to afford the required material; in short, that pyæmia occurred in dogs under circumstances very similar to those which determined it in human beings; and exhibited similar symptomatical and pathological aspects. A series of experiments were therefore commenced in January last, having for their object to acquire a knowledge of the morbid poison, and particularly to discover by what conditions the variations of its intensity were governed.

With reference to these experiments, Dr. Sanderson would not anticipate the complete account of them in the Report of the Medical Officers of the Privy Council, but would confine himself to giving an account of one series, and exhibiting the action of the pyæmic poison during life, and the *post-mortem* appearances; but before doing so, he would state shortly what he understood to be the true signification of the term pyæmia. He then proceeded to say:—

"The word pyæmia is apt to be used in somewhat different senses, according as the person using it has before him the medical or surgical aspect of the disease. To define it completely we must, I think, take into account its mode of origin, its symptoms, and the anatomical changes which it produces; not confining our attention to either of these to the exclusion of the rest. With this consideration in view I would comprehend in my definition the following propositions:—

"1. Pyæmia originates by the introduction into the living tissues, and eventually into the blood, of a *poison* which is itself a *product of inflammation*.

"2. The action of this poison manifests itself in an alteration of the blood, and in disorder of the vital functions. The former of these is characterized by the presence of bacteria, and by change in the optical characters of the blood, which often becomes obviously more transparent and darker by reflected light than it is naturally. Of the latter, viz., the general disorder of the vital functions, the most prominent phenomenon is fever, which, in the more intense forms of the affection, is followed by collapse which culminates in death.

"3. More remotely, the disease manifests itself in secondary suppurations, *i.e.*, in the formation of metastatic abscesses, which may occur either in the internal organs or underneath the skin. The special characters of these metastatic (as I am in the habit of calling them) infective abscesses are those which are well known both to surgeons and physicians. They have the additional less known character, that the pus they contain is full of bacteria.

"Pyæmia *differs* from tuberculosis in the rapidity of its progress, and in the obvious character of the anatomical changes of which it consists. Whereas, by tuberculosis we are understood to mean anatomically the overgrowth of cells in certain tissues, which we designate lymphatic on account of their proved anatomical relation to the lymphatic system, the secondary inflammations of pyæmia result in the formation of infective abscesses.

"Pyæmia resembles tuberculosis in its mode of origin. Both spring from inflammations; and, so far as relates to the anatomical characters of the lesions, both are inflammations. To both, therefore, the term secondary or infective inflammation is applicable.

"So much for the disease itself. Let me now," says, Dr. Sanderson, "draw your attention to the nature of the poison. I wish to show (1) that every pyæmic abscess contains a poison, which, when introduced either into the circulation or into a serous cavity, produces the symptoms of pyæmia; and (2) that we have this poison so entirely in our possession, and so far under control, that, beginning with an agent so mild in its action that it produces no marked symptoms, we can convert it into an agent of such intensity that it kills in two or three hours with the formidable symptoms seen in the case we have now before us.

"This intensification is effected by a process which may be called cultivation. Dr. Klein made the important discovery that, if a pyæmic liquid were transferred to the peritoneum of a guinea-pig, and allowed to remain there for a couple of days, although it did not at first produce any intense symptoms in the animal itself, its toxic intensity increased in such a degree that, when the transudation-liquid produced in this was injected into another animal, it had acquired the most deadly activity; and that all such extremely active liquids were crowded with bacteria of a peculiar character, the increased number of which seemed to be in proportion to their toxic properties."

Dr. Sanderson then proceeded to exhibit a dog, into the abdominal cavity of which six drops of a pyæmic transudation-liquid had been injected three hours before. The animal was in a state of profound collapse, accompanied with vomiting, purging, and cramps of the extremities. Shortly afterwards, the animal was killed and the abdominal cavity opened. The peritoneum con-

tained liquid slightly stained with blood, which, on microscopical examination, was found to be crowded with bacteria. The intestines were distended with a frothy liquid, which possessed none of the characters of the natural contents which had been found in other cases to be charged with shed epithelium. The internal surface of the whole of the alimentary canal from the stomach downwards was intensely infected, and presented appearances which (as had been found by more careful investigation in previous cases) were due to the separation of the epithelium from the surface of the mucous membrane, and the infiltration of that tissue with liquid.

The material which produced these results was obtained as follows: Pus from a pyæmic abscess of spontaneous, *i. e.* accidental, origin was introduced into the peritoneal cavity of a guinea-pig and allowed to remain there for two days. It was then withdrawn from the guinea-pig and some of it at once injected into the peritoneum of a dog. The dog was affected in exactly the same way as the animal exhibited to the Society. The remainder of the liquid was kept for five weeks in hermetically sealed tubes, after which six drops were injected into the peritoneum of a guinea-pig; this showed its action to have become relatively feeble. After two days, the transudation-liquid produced was (on the day before the meeting) tested with a third guinea-pig and found to be extremely active. This afternoon it was injected into the peritoneum of the dog exhibited.

Dr. Sanderson then concluded by saying: "Such are the facts. The all-important question remains—Do these experiments concern us as physicians and surgeons, or not? I think they do. But what I want is to prove it; for I am well aware, that unless clinical observation comes in aid of pathological experiment, the results of the latter do not tell practically. Let me state what are the lines of inquiry which I desire to see taken up. The first question is—Do the characters which we have shown to be present in the products of acute secondary inflammation in the lower animals, also exist in similar products in man? The second is more important still—Can it be shown that human pyæmic products, when tested by inoculation, possess exactly the same morbific properties as those which are possessed by the liquids to which our experiments relate? It is for answers to these inquiries that I earnestly ask the assistance of hospital surgeons.

"Finally, I would say a word as to the limits of the question now before us. With regard particularly to the question of bacteria, I desire to keep to the bare facts of disease, and not to diverge into discussions as to their origin. It is a matter to me of comparative indifference how they originate. Our observations lead us to conclude, first, that they afford a *characteristic* by which we may distinguish the products of infective inflammations from those which are not infective, and that their number affords an indication of the degree of infectiveness; and, secondly, that their presence in the blood is an indication of that constitutional disturbance which accompanies infective inflammation; not merely when that disturbance assumes the degree of intensity of which we have an example before us, but in the slighter form of irritative fever. If these facts prove to be true, not only in the lower animals but in man, their importance is quite unaffected by any theory which we may entertain as to the origin of bacteria."

After some remarks by the members, Dr. S. replied shortly that, as regarded bacteria in general, he was well aware from his own experiments that the ordinary bacteria of putrefaction possessed no toxic action, and that liquids containing them could be injected into the circulation of living animals without result. As regarded the bacteria of pyæmic products, he had carefully guarded against the inference that they were the efficient causes of pyæmia. He regarded them as only characteristic inhabitants of infective liquids, and therefore very probably carriers of infection. As regarded the word septicæmia, he understood it to mean a state of the blood which was only present in the most intense forms of pyæmia; and he entirely agreed with Mr. Hulke in regarding metastatic abscesses as an accident rather than as an essential of pyæmic infection. The theory that the pyæmic poison was dependent upon an alkaloid, would be disproved in case it should appear that it was incapable of diffusion.

On this question, further inquiries were necessary.—*Brit. Med. Journ.*, May 11, 1872.

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23. *Scleroderma Adultorum.*—Dr. J. Neumann calls attention to this remarkable form of cutaneous disease. Though individual cases were recorded by older writers, no special essay upon it appears to have preceded that by M. Thirial in 1845, who published in the *Gazette Médicale* an account of all the cases he had been able to collect, and applied the term "sclerema" to the affection. Since then no less than fifty cases have been reported, and M. Neumann himself adds three more to the list. In most of the cases the skin becomes gradually harder and more tense, though in some instances, and especially when the attack is acute, there is a preliminary stage of œdema. Hebra and Rasmussen distinguish two forms of the disease—"Sclerema elevatum" and "S. atrophicum," of which the former is the acute and the latter the chronic type. The disease has been preceded sometimes by rheumatism, sometimes by recurrent attacks of erysipelas, and occasionally it has appeared to originate in injury; but little is really known of its etiology. The symptoms are very variable; but there is always the disagreeable sensation of tension, impeding movement to a greater or less extent. Thus, if the sclerema affects the neck, the rotation and flexion of the head are interfered with. In sclerema of the face, the facial expression is lost, the natural lines and furrows are smoothed out, and the play of the features is lost; the eyelids droop; the alæ nasi are stretched, and the nose is flattened; the oral opening is contracted, and the movements of the lips rendered difficult. With regard to other parts of the body, the abdominal wall is made tense; the scrotum and penis so tense that no erection can occur; the elbows are bent: the fingers half extended and claw-like; the spinal column is bent forward, and the respiration even impeded. In one of Dr. Neumann's cases violent itching was complained of, and in this case the temperature was considerably elevated, though it is usually depressed. The secretion of the sebaceous follicles does not wholly fail, since pustules of acne have been observed to form. In some instances the secretion of sweat seems to be entirely arrested, but not in others. The disease has been observed to be associated with erythema, erysipelas proceeding to vesication, impetigo, ecthyma, eczema, partial teleangiectasis, ulcerations, and acne. It is more common in females than in males, and the majority of cases have occurred between the ages of twenty-five and forty, though one case is recorded in a patient aged six, and in another aged seventy-two. The pathological conditions of the skin that have been discovered under the microscope are hypertrophy and condensation of the subcutaneous cellular tissue, with coincident atrophy of the adipose tissue, deposit of pigment in the rete Malpighii, around the vessels, in the cellular investment of the several ducts, in the outer-root sheaths of the hair, and in the sebaceous follicles. The pigmentation, however, appears to bear no relation to the intensity of the disease, the densest parts being often the lightest colored. In one of Dr. Neumann's patients the epidermis was found to be unaltered, the cells of the rete Malpighii were increased in number, and formed wart-like processes, dipping into the cutis, not pigmented. The fibres of the cutis formed large and strong bands that preserved the characters of connective tissue, and were intermingled with thick trabeculæ of elastic tissue. Collections of cells occurred in the deeper parts of the corium, and in the panniculus adiposus, the fat of which was much atrophied. The sweat-glands were individually much hypertrophied, and the smooth muscular fasciculi were also much increased in size. The vessels running in the cutis were large and filled with blood, but the branches running towards the papillæ were small and empty. The hairs were small and woolly, but the sebaceous follicles were large. No treatment has been hitherto suggested that possesses any value in curing, or even in staying the progress of the disease.—*Lancet*, March 9, 1872.

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24. *Pathology of Herpes Zoster.*—Dr. O. Wyss had the opportunity of carefully watching during life, and examining after death, a patient of von Witte's, of Rheinau, suffering from herpes. The patient was sixty-eight years of age, and on September 16th he had headache and febrile symptoms. On the 19th scattered

vesicles appeared of herpes labialis. On the 20th the right side of the forehead, and nose and cheek as far as to the border of the lower jaw, were injected. On the 22d the right eye itself was inflamed, and on the 23d the left. Two days afterwards an eruption of herpes appeared on the right side of the face, which affected the cornea and conjunctiva. On the 28th the patient died. The post-mortem examination was conducted with great care. The herpetic vesicles and scabs were very accurately limited to the right side, and to the parts supplied by the first branch of the trigeminal nerve. The left eye was perfectly normal. The nerve above mentioned was found to be broader and thicker than that of the left side, of a deeper gray-red colour, of softer consistence, and with the several nerve fasciculi separated by grayish-red soft tissue, containing many vessels. This alteration in its character extended from the point where it entered the orbit to the finest branches as far as they could be traced with the simple lens. The other nerves traversing the orbit were perfectly healthy. Outside the orbit and extending from it to the ganglion Gasseri, the first branch of the fifth was surrounded with extravasated blood. On the proximal side of the ganglion Gasserianum the fifth nerve was normal in appearance, The ganglion itself was larger and somewhat more succulent than the left; upon its inner side was a red mass that appeared to be caused by an ecchymosis. The proper substance of the ganglion was not of a yellowish-white colour. but bright red. The fifth nerve was healthy at its apparent origin from the brain, where it entered the Gasserian ganglion. There were numerous ecchymoses. These were especially visible also in that part of the ganglion whence the first branch of the fifth arises, whilst that from which the second and third branches arise was little altered. Microscopical examination of the skin showed that the papilla and the corium were strongly infiltrated with cells. In some parts the rete was preserved, in others, together with the upper layer of the corium, destroyed. The fasciculi of connective tissue on the forehead exhibited an infiltration of cells, especially in the vicinity of the vessels of the sweat-glands; where the cutis was most deeply ulcerated these fasciculi presented a lustrous, homogeneous character, corresponding to the vitreous swelling of Neuman, or hyaline degeneration of O. Weber. The cornea was abundantly infiltrated with cells, especially in its upper layers; the ulceration of the surface penetrated to the substantia propia. This is an important communication, and shows conclusively that herpes zoster is a consequence of inflammation of the corresponding spinal ganglion and of the nerve traversing it.—*Lancet* May 4, 1871.

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25. *Alcoholic Paresis and Paraplegia.* By J. LOCKHART CLARKE, M.D.—The habitual and excessive indulgence in the use of alcoholic drinks is so frequently followed by partial or complete paralysis of the lower extremities, that no doubt can be entertained that alcohol and paraplegia do often stand to each other in the relation of cause and effect. Many striking cases that have come under my own care support this inference in a very convincing way, so that I think Drs. Handfield Jones and Wilks have done good service in bringing the subject prominently before the profession.

On examining the bodies of persons who have died either in a state of intoxication or during a course of excessive indulgence in the use of alcohol, particularly in its undiluted state, almost every organ and tissue is found to be more or less altered in appearance. The mucous membrane of the pharynx, œsophagus, stomach, small intestines, and bronchial tubes is red and injected. The liver and kidneys, substance and membranes of the brain and spinal cord, are in a variable state of congestion. In chronic cases of alcoholism we find more or less atrophy of the cerebral convolutions, and effusion of fluid on their surfaces, with thickening and adhesions of the membranes.

On examining such persons during life, we find a train of symptoms, that are quite in accordance with these post-mortem appearances. The mucous membrane of the fauces is unusually red and injected; a viscid, tenacious, or cream-coloured mucus hangs about the back of the pharynx and soft palate, exciting vomiting on rising in the morning; a sensation of rawness or heat is sometimes felt along the œsophagus, particularly on drinking warm or spirituous fluids;

the stomach is irritable, and vomiting is easily excited. A viscid and tenacious mucus collects during the night on the membrane of the bronchial tubes, causing more or less shortness of breath, or oppression of the chest on waking in the morning, and exciting cough, which is frequently attended by vomiting. The skin has frequently a dirty yellow tint, although sometimes a remarkable clearness and freshness of color is preserved, especially in fair persons. The conjunctivæ are often thick, injected, and dirty-yellow.

Different individuals suffer from a variety of nervous symptoms—from giddiness; a sensation of being lifted from the ground, or "taken off their legs," as I have heard them express the feeling; an alarming sensation of choking or suffocation on dropping off to sleep at nights; occasional numbness or "pins and needles" in the fingers and toes; or acute pains beneath the nails; a dull, aching pain across the loins, or an acute pain on bending or on rising from the sitting posture; a darting or lacerating pain in the lower extremities during the act of progression.

The muscular system is more or less affected by weakness, by spasm, or by the withdrawal of voluntary control. The hands and arms, and sometimes the whole body, are tremulous, with frequent fibrilla quivering of the muscles, particularly about the face. Voluntary movements are imperfectly or awkwardly performed. Even when there is no tremour of the hands, delicate operations—such as writing—are clumsily performed, and are irksome or distressing. The walk is more or less unsteady, and I have often noticed that in progression each foot is alternately carried inwards, and almost in front of the other. The articulation is sometimes peculiar and indistinct from partial loss of control over the muscles of the lips. The individual loses his accustomed energy, is careless of his own interest, and feels himself unfit for the performance of his ordinary duties, until he has resorted to his usual potation, or obtained from the druggist a "pick-me-up." For this distressing state I have found nothing so useful as full doses of nitro-hydrochloric acid, with a little of Battley's sedative solution; two or three doses, even, sometimes act like a charm. After relieving the bowels by saline aperients, I combine the acid with quinia, perchloride of iron, or small doses of strychnia. The occasional use of the Turkish bath is also beneficial.

If the individual persists in the excessive use of alcoholic drinks, partial or complete paralysis, and particularly paraplegia, not unfrequently results.—*Lancet*, March 30th, 1872.

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26. *Leucocythæmia accompanied by Alteration of the Marrow of the Bones.*—E. Neumann relates the following case:—The patient, æt. 30, presented during life all the symptoms of splenic leucocythæmia; he died from repeated hemorrhages. After death there was found considerable hypertrophy of the spleen and liver, and the usual alterations of the blood. The parenchyma of the spleen and liver presented all the characters of leukæmic hyperplasia; the liver also enclosed a whitish nucleus of some size, in which a collection of lymphatic cells had taken the place of the hepatic cells. The marrow of the bones presented very curious lesions, especially observed in the ribs, the sternum, a vertebra, the diaphysis of the humerus, and the diploe of the cranium. At all these points the marrow presented a uniform greenish yellow colour, traversed by a few small red veins; it had a ropy consistence and resembled creamy pus. The microscope revealed white corpuscles of the blood, of varied form and in countless numbers—perhaps, in the course of transformation into red corpuscles. These elements were inclosed in a filamentous substance, containing mucine and very slightly vascular. The bloodvessels were represented by some isolated arterioles. The walls of the larger of these vessels were infiltrated with lymphatic corpuscles; those of the little arterioles were formed by slender fusiform cells, elongated as in the spleen. These vessels were almost exclusively filled with red globules. The author proposes that a myelogenic leucocythæmia should be admitted in addition to a splenic and lymphatic. The capillary network being wanting, the blood is brought by the arteries and thrown directly into the pulp of the marrow, which is rich in cells, and it is probably returned into the venous canals mixed with the elements of this tis-

sue. The presence in the blood of red globules incompletely developed, only met in the marrow of the bones in health, is also explained.—*Archives Gén. de Méd.*, February, 1872, from *Arch. der Heilkunde*, xi., 1–15.

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27. *Caries of the Vertebræ and Disease of the Cord.*—Dr. C. FROMMANN records (*Virchow's Archiv*, December, 1871) a case of this, the primary affection in which was caries, beginning in the eighth dorsal vertebra, but extending to the seventh and sixth, which were found affected in a minor degree. The spinal cord was secondarily affected, its condition after death being analogous to that seen in tabes dorsalis, and to which, from its anatomical character, the name gray degeneration has been applied. The first symptoms of disease of the nervous system were pains in the region of the nerves which proceeded from the diseased vertebræ or their neighbourhood. After these pains had existed for a year, pain and paralysis of the lower limbs appeared; and the paralysis became complete within a few weeks of its first appearance. Along with the affection of the lower limbs, the arms presented involuntary contraction of individual muscles, and of the adductors. In the lower limbs, in addition to the motor paralysis, there was also marked diminution of sensation; pricking, pinching, or pressure on the skin being only indistinctly, or not at all felt. The application of a wet sponge to the legs was not appreciable to the patient till the temperature of the water had been raised to 45° C. (113° F.). Reflex irritability was increased, movement being elicited by pricking or pressure, but especially by the application of a sponge containing water at an elevated temperature. These movements occurred several seconds after the irritation, and a still longer interval elapsed before the patient became conscious of them. After seven weeks bed-sores formed, and the patient succumbed to these. The symptoms described resemble considerably those of myelitis, and it was expected that the inspection would reveal inflammation of the cord, communicated through the membranes from the diseased vertebræ. It was found, however, that the pia mater presented only the slightest degree of inflammation, and there was no trace of any communication of inflammation from the dura mater to the pia mater? The meninges being thus practically free, it appeared that the disease consisted of an irritation of the bloodvessels and the connective substance (neuroglia) of the cord itself. The neuroglia was swollen, and had begun to undergo fibrillation, though the affection was comparatively slight in degree, and the nerve fibres very little destroyed. The disease was thus in a comparatively early stage, but still presented the pathological changes found in gray degeneration. The distribution of the disease was, however, considerably different here from what is usually seen in gray degeneration, the lateral columns being simultaneously affected with the posterior, whereas, it is well known that in gray degeneration it is usually the posterior columns alone which are involved. To this, as well as to the fact that the disease had such a rapid course, the author ascribes the great difference in the symptoms from what is usually seen in gray degeneration of the cord.—*Glasgow Med. Journ.*, May, 1872.

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28. *Treatment of Dropsy.*—Dr. F. T. ROBERTS, in an instructive paper on this subject (*Practitioner*, May, 1872), calls attention to the following measures which may be employed with advantage in different cases, and in many two or more can be combined. 1. The maintenance of *rest* and of an *appropriate position*. 2. The use of baths, both *general* and *local*. 3. The employment of regular and systematic *pressure*. 4. Removal of the fluid by *operation*. One or other of these may be employed with advantage in different cases, and in many two or more can be combined.

1. The object of *rest* and *position* is to place the parts affected in a posture unfavourable to the action of gravity, so that the veins may not be over-distended, or the fluid tend to accumulate in dependent parts, and thus, with the aid of rest, to promote the absorption of what has already collected. It is in the case of anasarca of the legs, and œdema of the scrotum, that attention to this point proves most serviceable.

2. He advocates having recourse to the *regular* employment of some effec-

tual bath if it is desired to excite the excretory functions of the skin sufficiently to produce any marked benefit. If it can be used, a general bath of course acts best, and either the hot-air or vapour bath is to be preferred; it is especially in the general dropsy of Bright's disease that these are valuable. In many cases, however, especially in cardiac affections, the patient cannot bear a repetition of either of these, and under these circumstances I have found marked benefit from the application of local heat and moisture in the following simple manner, by which the parts are kept in a perpetual local bath. Each leg is wrapped up, from the hip down, in a large flannel, wrung as dry as possible after having been dipped in very hot water, and this is enveloped in a piece of mackintosh of sufficient size, which prevents evaporation and cooling; the flannels are changed about every hour, care being taken to avoid chilling the patient, and the legs are well dried before a fresh application is made. I have often been surprised at the very satisfactory results of this plan, both in hospital and private practice, if persevered in for two or three days, and there is no difficulty, as a rule, in following it out.

3. Considerable advantage may be obtained by the employment of steady and continuous pressure, applied by means of bandages, or some elastic apparatus, care being taken that it is made properly and uniformly, varying its amount according to the nature of the case. Not only is it useful in itself, but it materially helps another mode of treatment, hereafter to be considered.

In local dropsies, the result of venous obstruction, this does more good, he says, than anything else.

4. The early and, if necessary, repeated performance of *paracentesis abdominis* in appropriate cases of ascites is strongly advocated.

Puncturing the legs and scrotum, when considerable œdema exists in these parts, is also recommended. In cardiac dropsy a few punctures, repeated for some days, may, he says, give material help in removing the fluid altogether, at all events for a time, by relieving the over-distended vessels, and thus enabling them to absorb. Of course permanent benefit is not to be expected in these cases, but it is a great thing to relieve the very unpleasant feelings associated with this form of dropsy. In the case of the legs, it is below the knee that the punctures should be made, as, if they are made above this point, urine may come into contact with them, and lead to erysipelas. It is unnecessary to make large incisions, the punctures produced by ordinary harelip pins answering very well. Several may be made at intervals in dependent parts, also on the dorsum of the foot, if required; and they may be repeated, if necessary, so long as there is no sign of irritation. It is advisable to wrap up the limbs in cotton-wool and flannel, which should be frequently changed. As regards the scrotum, this may be punctured in several points on both sides, and then well fomented. Great care must be taken to keep this part clean. In some cases the operation causes it to become indurated, and this condition resists the further accumulation of fluid.

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29. *Relative Efficacy of Tincture of Hyoscyamus, Bromide of Potassium, and Chloral, in single doses, on Maniacal Excitement.*—Dr. John A. Campbell, of Garland Asylum, Carlisle, states that being accustomed to give some of the excitedly noisy, the restless, and other cases labouring under insomnia, as well as the recent acutely-excited cases, some sleep-causing medicine nightly, and having frequently given the above-named medicines, he thought that if he gave them for some time in the same doses to the same patients, he might be able to form a tolerably accurate idea of their relative efficacy in the following respects: 1. Which was the surest sedative to excitement? 2. Which was the most certain sleep-producer? 3. Which acted most quickly? 4. What is about the equivalent dose of each? The method he adopted to arrive at the result he wished to attain was, that to all the excited, and then to all the simply sleepless patients, whom he could get to take medicine, he gave on consecutive nights two drachms of tincture of hyoscyamus, one drachm of bromide of potassium, and thirty of chloral, frequently repeating these experiments. He considered that by giving the medicines on alternate nights, if the patient's taste changed, it would do so for all the medicines; and if any of the medicines had a cumulative ten-

dency, it would not show itself. The number of cases selected was fourteen. To these he gave each of the medicines forty-six times. The total results he got were, that tincture of hyoscyamus produced sleep on forty-two occasions, bromide of potassium on thirty-eight, and chloral on forty-three occasions. The average time in which effects were observed with tincture of hyoscyamus was 1 hour and 7 min., with bromide of potassium 1 hour 15 min., and with chloral 48 min. The average time the effects lasted with tincture of hyoscyamus was 4 hours 6 min., with the bromide, 3 hours 54 min., and with chloral, 4 hours 26 min. Thus it appears that thirty grains of chloral produced effects more quickly, sleep more frequently, and its effects lasted longer than two drachms of tincture of hyoscyamus, and this dose of tincture of hyoscyamus was more powerful in these respects than one drachm of bromide of potassium. In a second series of experiments, Dr. Campbell increased the dose by one-half, that is he gave of tincture of hyoscyamus, ʒiij.; of bromide of potassium, gr. xc; and of chloral, gr. xlv. He only gave this dose, however, to the more excited of the patients. The total results he arrived at were (each of the medicines having been given on thirty-one occasions) that tincture of hyoscyamus produced sleep on twenty-four occasions, bromide of potassium on eighteen, and chloral on twenty-eight occasions. The average time in which effects were observed with tincture of hyoscyamus was 35 min.; with bromide of potassium, 46 min.; and chloral, 29 min. The average time the effects lasted with tincture of hyoscyamus was 3 hrs. 39 min.; with the bromide, 3 hrs. 31 min.; and with the chloral, 4 hours 27 min. Chloral in this dose, as in the former, shows its superiority, having produced its effects more quickly, sleep more frequently, and its action continuing longer than either of the other medicines. Tincture of hyoscyamus retains its place next to chloral. Finally, in a third series of experiments, he increased the dose to double that with which he had begun. He gave these doses in two cases only—to one of them on five occasions, to the other on one. In this dose again chloral proved more active than tincture of hyoscyamus and the bromide proved utterly inefficacious where so much excitement existed. The results of these experiments, which, however, from their limited number, he considers ought not to be regarded as conclusive, are—1. That both chloral and tincture of hyoscyamus are sure sedatives to maniacal excitement. 2. That of these two medicines chloral is the most certain sleep-producer. 3. That chloral acts more quickly than tincture of hyoscyamus. 4. That though bromide of potassium, in such doses, is a sedative to maniacal excitement, and to a certain extent hypnotic, yet it is not a sufficiently powerful sedative to allay intense excitement, or an hypnotic to compel sleep where great insomnia exists. 5. That a two-drachm dose of tincture of hyoscyamus is not quite equivalent to thirty grains of chloral. Two and a half drachms would probably be as nearly an equivalent as could be given. From the different sedative and soporific powers of bromide of potassium, he thinks it is hardly possible to form an idea of an equivalent dose. It appears to him to be useful only when the excitement or insomnia is of a slight character.—*Practitioner*, April, 1872, *Journal of Mental Science*, Jan. 1872.

30. *Treatment of Pneumonia by Acetate of Lead.*—Dr. STROHL, of Strasburg, strongly recommends the use of acetate of lead in pneumonia; and, discussing the whole subject of the treatment of that disease, he comes to the conclusion that lead is the safest and best remedial agent. He compares the results obtained by different modes of treatment, such as the expectant plan, bleeding, emetics, mixed treatment of bleeding and tartar-emetic, etc.; and he claims for his own plan the most satisfactory results, for the ratio of mortality in the cases he treated was only 1 in 9.66; but, even if the mortality were the same, he claims for lead certain advantages which it possesses over other remedies, and especially its innocuity and the rapidity of its curative action. Tartar-emetic causes vomiting, gastro-intestinal irritation, and prostration, but acetate of lead does nothing of the kind, and Dr. Strohl found that it did not even cause a threatening of colic. Again, the objections to other modes of treatment are rather numerous; bleeding is inapplicable in the extreme of age and in bad constitutions; tartar-emetic is contra-indicated in cases accom-

panied by inflammatory or other serious affections of the stomach and intestines, and in diarrhœa and phthisis; but in all these cases lead may be given. Dr. Strohl sums up his views as to the efficacy of acetate of lead in the treatment of pneumonia to the following effect: It is preferable to tartar-emetic, digitalis, and veratria, because its action is more certain, more rapid, and more free from inconvenience, and it has never caused any bad symptoms. It never constipates, but sometimes causes diarrhœa, and it may be given to persons of all ages. Under its use the pulse is lowered, and the heat and fever are reduced, and convalescence is very rapid when there are no persistent complications.—*Brit. & For. Medico-Chir. Rev.*, April, 1872, from *L'Union Médicale*, October 12, 1871.

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31. *Pepsine Wine in Diarrhœa of Young Children.*—Dr. A. Davidson extols (*The Practitioner*, March, 1872) the efficacy of pepsine wine in the diarrhœa often observed in children from one to two years old, resulting from feebleness of the digestive powers, and in which a large proportion of the food is not assimilated, but passes off in an undigested state. Aromatic and astringent mixtures are useless in these cases, but Dr. D. says that pepsine wine given in doses of a teaspoonful three or four times a day with the food, soon effects a cure. This statement, though it has no claim to novelty, is interesting as confirmatory of the observations of Dr. J. C. Reeve, of Dayton, Ohio, published in the number of this Journal for July, 1865, pp. 55–58.

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32. *Treatment of Diabetes Insipidus.*—M. Guenéau de Musy, in a clinical lecture at the Hôtel-Dieu, recommends the administration of full doses of belladonna and sulphurous baths, in the treatment of diabetes insipidus. He has twice found belladonna to accidentally produce anuria. Its use in incontinence of urine is well established. Systematically employed in diabetes insipidus, it has diminished the quantity of urine passed from ten pints to two pints *per diem*. The sulphurous baths bring the skin to the relief of the kidney.—*Brit. Med. Journ.*, June 1, 1872.

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33. *Xylol in Smallpox.*—Dr. Russel relates (*Glasgow Med. Journ.*, May, 1872) a number of cases of smallpox treated by him with xylol as recommended by Zuelzer, of Berlin, and Burkart, of Stuttgart, and in none of them did it produce the slightest obvious effect on the progress of the disease.

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34. *Glycerined Vaccine Lymph.*—E. Muller, Director of the Berlin Vaccine Institution, adverts (*Berlin Klin. Woch.*, April 29, 1872) to the large scale in which revaccination had to be carried on during the late war, where so many troops had to be protected against the smallpox prevailing in the enemy's country, while vast numbers of French troops were "interned" in Germany. Before the introduction of this new plan the military surgeons had often very great difficulty in obtaining the requisite lymph. They obtained as much as they could from the establishments, and then revaccinated from the finest revaccination pustules that presented themselves. Some danger was incurred thus of also transmitting disease, while the lymph derived from revaccination is supposed to possess less protective power.

Military surgeons are saved all trouble now on this score by the employment of glycerined lymph. Six years have now elapsed since the discovery of its advantages, and Dr. Muller, during this period, has been untiringly endeavouring to make these known more widely. The late war, occurring amidst the prevalence of smallpox, has amply demonstrated its value; for it was found that, on the outbreak of the disease, the only provision in Germany of lymph capable of meeting the emergency existed at the Berlin Institution, and supplies had to be obtained from it for revaccination in Baden, and for revaccinating the Saxon and Wurtemberg armies. Without the aid of glycerine the immense supplies for both civil and military practitioners, at home and abroad, never could have been sent; and the army surgeons can best judge what influence the withholding of such revaccination might have had on the course of the campaign. Still, in general, only enough lymph could be sent to set the revac-

cination process going, the revaccinating from arm to arm still continuing to be practised. In fact, but few military surgeons prepare the glycerined lymph themselves, although this can be so easily done. If during the summer each were to vaccinate a few healthy children, he might lay by a stock of glycerined lymph (which keeps for months or years) that would enable him to revaccinate whole batches of recruits on the same day.—*Med. Times and Gaz.*, June 1, 1872.

35. *Reno-vesical Cantharidism, and the Remedies in general use for its Relief.*—M. GUBLER remarks that the noxious influence of cantharides is especially marked on the kidneys and bladder, the characteristic symptoms being well known. Morel-Lavallée distinguishes three forms of cystitis. The first, which is the simplest, is recognized by the presence of albumen in the urine; in the two others, which only differ in minor points, there are pains in the perineum and abdomen, with strangury and the discharge of false membranes. He thought that the active principle of cantharides set free by the acidity of the urine occasioned vesication of the bladder: but M. Bouillaud gave the true pathognomy of the symptoms observed. He showed that the topical action of cantharides is on the kidneys, of which the internal membrane experiences its vesicating action, in consequence of which the urine becomes charged with albumen. It is really an endonephritis, and not a cystitis, that constitutes the point *du départ* of the albuminuria produced by cantharides. If the renal irritation becomes more severe, a nephritis parenchymatoso is established. It is astonishing that a substance which proves so irritating to the tubuli of the kidneys should be without effect on the bloodvessels. But M. Gubler shows that the albumen of the blood plays an important part in preventing the production of the usual effects of this and similar substances by seizing upon and forming combinations with them, so that their powers are rendered latent and are only manifested when set free by the renal gland into a fluid that contains no albumen: and he considers this to furnish a truer explanation of the peculiarities presented by cantharidine in the blood and in the urine respectively than that advanced by M. Bouillaud, who believed that whilst in the blood the cantharadine was rendered inert, owing to its combination with some alkali, as soda; for M. Gubler has satisfied himself that the combinations of cantharidine both with potash and soda are possessed of considerable activity. Important corollaries flow from this proposition of M. Gubler's, that cantharidism is neutralized in the vascular system by the proteic matters of the blood: thus it is obvious that it is useless to prolong the application of a blister after the vesicle has risen, since, starting from this point, its contact ceases to be immediate, and the solution of the cantharidine in the albuminous or albumino-fibrinous serum loses its irritating properties. Hence also cantharidine exerts its irritant action on all the emunctories which discharge fluids destitute of albumen, such as sweat and tears, and perhaps too the parotid glands. On the other hand, it escapes unperceived into all serous cavities, and probably into the submaxillary and sublingual glands, the saliva of which is viscous and stringy. In M. Gubler's wards, 176 blisters were applied in one year, and in 16 instances more or less marked symptoms of cantharidism were observed, or 1 in 11; but in four of these the symptoms were very slight indeed. In one only was there severe hæmaturia. He does not believe that camphor possesses the slightest power of preventing the injurious effects of cantharides.—*Practitioner*, April, 1872, from *Bulletin Général de Thérapeutique*, Dec. 30, 1871.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

36. *Adhesion between Prepuce and Glans Penis in Children.*—We learn from the *Jahrbuch f. Kinderheilkunde* that the investigations of JOH. BÓKAI regarding what he terms cellular adhesion between the prepuce and glans penis in children, were first published in January, 1860, in the Hungarian language.

A full exposition of the results of those investigations are interesting from their important practical bearing. This exposition we borrow from the *Centralblatt für die Medicinische Wissenschaften*, January 13, 1872. M. Bókai refers the prepucial adhesion of which he treats to almost entirely physiological causes, and only under especial circumstances admits it to be strictly pathological. The origin of the adhesion is, he believes, to be laid in the fœtus by the close and undisturbed contact which then exists between the superficial cells of the lining epithelium of the prepuce and the surface of the glans. They are in this condition slower in hardening. This process is, likewise, interfered with by the cells of the epithelial layer of these parts being more constantly and fully bathed in fluids than elsewhere, which favours the adhesion of the opposing surfaces. This condition of things continues to a greater or less extent, in different cases, for some time after birth; but diminishes with the advance of the child's age.

In 100 children, carefully examined by Dr. B., there were 30, aged from five *hours* to six *months*, in 22 of which the adhesion was of the highest degree; 20 were from 7 *months* to 1 *year* old; in 9 there was extensive adhesion; 15 of the boys were between 1 and 2 years old, of whom 5 exhibited a high degree of adhesion; 16 were between 2 and 3 years old, adhesion prominent in 3; among 11, from 4 to 7 years old, one only exhibited extensive adhesion, while in 8 boys, from 9 to 13 years old, no degree of adhesion was present. In all the remaining cases there was adhesion but to only a slight extent. Dr. B. divides the adhesion formed between the prepuce and glans into *three* distinct grades. 1. Where the adhesion, commencing at the *corona glandis*, extends thence backwards, general with normal width of prepuce, but often also with an abnormal narrow and short prepuce. 2. Adhesion extending from the centre of the glans backwards; with normal width and length of prepuce. 3. Adhesion extending from the apex of the glans to the sulcus beneath the corona glandis. In new-born infants, in such cases, the external orifice of the urethra is often found to be closed. In all the three grades of adhesion pain is caused when an attempt is made to retract the prepuce beyond the line at which the adhesion commences, and the disruptured surfaces present a torn and reddened appearance, even a discharge of blood may be the result when the adhesion has been very intimate. In the sulcus behind the corona glandis there is always a considerable collection of a cheese-like matter, the removal of which is sometimes attended with difficulty. It is the presence of this cheese-like matter, according to Dr. B., which causes the pulling at the penis so often observed in children, and from which the superficial cracks so often seen on a line with the adhesion result, and from which blood has been known freely to escape, and a circumscribed inflammation produced at the edge of the prepuce. Finally, there is painful micturition, a symptom which may be the first to call the physician's attention to the existence of the adhesion of the prepuce. In regard to the treatment of the several grades of the adhesion referred to by surgical interference, Dr. B. remarks, that inasmuch as there is a tendency to a spontaneous disappearance of the adhesion as the child grows in years, and his organs increase in bulk, the separation of the adhesion by the knife should never be resorted to excepting in cases where the adhesion has resulted from some strictly pathological cause, or there is retention of urine, or where *balanitis* has set in. By retracting the prepuce with one hand, and with the other, armed with a blunt sound, tearing apart the adhesions, all difficulty may be overcome. Dr. B. had never occasion to resort to any cutting instrument. In those in which there has taken place spontaneously a partial separation, forming a deep sac-like cavity, Dr. B. has succeeded in removing the remaining adhesion by the forcible injection of water into the cavity formed as above. The adhesion being entirely overcome, the cheesy matter should be removed from the base of the glans, both the surfaces that had been adherent, after being properly cleansed, should be anointed with olive oil or glycerine, and the prepuce brought forward and placed in its natural position. If any hemorrhage ensue from the disruption of closely adherent adhesions, it may be easily arrested by the application of cloths wetted with cold water. D. F. C.

37. *Treatment of Goitre.*—Dr. C. SCHWALBE has contributed to *Virchow's Archives* (December, 1871) a very interesting and practical paper on this subject. The results arrived at are based on 106 cases, of which notes were made. In order to the treatment of goitre, it is of consequence to decide as to the exact nature of the swelling, and so the author gives a short description of the various kinds chiefly with the view to diagnosis. There is first the glandular form (struma follicularis mollis of Virchow), which consists of a hypertrophy or hyperplasia of the thyroid gland, and which forms a soft elastic tumour, not unlike a lipoma. There is farther the cystic goitre, in which it is usually easy to detect fluctuation. The commonest remaining forms are the fibrous and the vascular, the former distinguished by its hardness. As an important aid to the diagnosis, the author nearly always resorts to puncture with a perforated needle, and the proceeding is of particular advantage in distinguishing between glandular tumours and cysts. The glandular tumours not unfrequently give a false feeling of fluctuation; and on the other hand it is often impossible to detect fluctuation though the tumour is cystic, and in these circumstances puncture is of great service, the fluid which issues when the disease is cystic at once determines its nature. The fibrous forms are also distinguished on puncture by the density of the tissue, and the vascular by the free flow of blood. In his treatment the author trusts in the first place to the internal administration of iodide of potassium, combined in almost every case with the local injection of tincture of iodine, or simple alcohol. In very recent cases the use of the iodide (he gives it to the amount of 1 gramme, or about 15½ grains daily) is sufficient to dissipate the swelling, but usually injection is also necessary, except in the vascular forms. In respect to the material to be used in injection, the author believes that the benefit derived from the injection of tincture of iodine depends chiefly on its alcohol, and that rectified spirit is in most cases of equal use. The internal use of KI or I is undoubtedly of great benefit, but there is no reason to believe that the iodine has any special effect when applied locally, and the author has found that the injection of alcohol with the internal administration of the iodide has as good results as the injection of the tincture of iodine. To the surgeon, there can be no comparison of the cleanliness and convenience of alcohol as compared with the tincture of iodine. The quantity injected varies in different cases, in the glandular forms only small injections were used, about 10 to 20 drops; and even smaller quantities in the fibrous. But in the cystic form much larger quantities were used, as much as 4 grains being injected into a cyst as large as a duck's egg. The author has not made much use of electrolysis in the treatment of goitre; and he employs it chiefly for the vascular and fibrous forms. Incision of the cystic form he has very seldom resorted to, and only where the tumour was very large. The plate which accompanies the paper is illustrative of a case cured by this method. In conclusion, it is stated that by these means he has only once failed to effect a cure, this case being one of the very hard fibrous form; at the time it was under treatment he had no constant current battery at hand, and he thinks it possible that he might have come to some result if that treatment had been used.—*Glasgow Med. Journ.*, May, 1872.

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38. *Method of instantly arresting Epistaxis.*—Dr. MARIN, of Geneva, states (*Journ. de Méd. et de Chirurg. Practique*, May, 1872) that as the blood in epistaxis generally flows from only one nostril, and most frequently from the anterior third of one of the nasal fossæ, he was led to believe that by compressing the corresponding facial artery on the superior maxillary bone near the ala of the nose, the afflux of blood would be diminished, and the hemorrhage at once be arrested. He has tried this plan in very many serious hemorrhages from the nose, and the expedient has proved perfectly and promptly successful. —*L'Union Médicale*, 25th May, 1872.

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39. *New Method of Treating Hydrocele.*—Mr. S. M. BRADLEY states (*Brit. Med. Journ.*, June 1, 1872), that as the serous tissue of the testicle is, from its physiological nature, liable to take on adhesive action, and that, from the character of the secretion poured out in a hydrocele being inflammatory and

not dropsical, it would even be prone to do so, he was led to the inference that simple tapping, followed by firm and equal strapping of the affected side, would probably be followed by an obliteration of the vaginal sac and a consequent radical cure. He tested the accuracy of this reasoning in the case of a medical man with a large simple hydrocele which had been tapped several times, and the last time injected with iodine without success. After explaining my object to him, I tapped the hydrocele, drawing off half a pint of fluid, and tightly strapped the affected testicle with soap-plaster. This was done at my own house, and the patient walked home, a distance of about a mile, immediately afterwards, and continued to go about during the process of recovery, which probably took place in about ten days; I say probably, as I kept up the pressure for three weeks without allowing the testicle at any time to remain unsupported. This case occurred eight months ago; since then I have followed the same course in three other instances, and in each with an equally satisfactory result. In no case was there any fresh effusion of fluid.

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40. *New Method of Forced Dilatation for the Treatment of Fissure of the Anus.*—Dr. Alf. Liegard, of Caen, describes (*La Tribune Médicale*, April, 28, 1872), the following method of forced dilatation for the cure of fissures of the anus, which he has successfully employed in cases which proved rebellious to various other methods of treatment. The patient being chloroformed, he introduces into the rectum a small bivalve speculum uteri, which when closed is only three centimetres in diameter, but when completely opened becomes eight centimetres. This is easily introduced when closed, and being then opened it can only be withdrawn by the use of considerable force. The resistance being overcome, the parts are distended and a cure is at once effected.

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41. *Strangulated Congenital Inguinal Hernia reduced by Aid of Aspiration.*—M. Demarquay exhibited to the French Academy of Medicine, May 21, a case of this in a young man, æt. 21. On May 5 a tumour appeared in the left groin, accompanied by severe pains and vomiting, which persisted next day. At the end of twenty-four hours he was taken to the Paris Maison de Santé, where the taxis was employed without success. Ice was applied during the next twelve hours, when M. Demarquay saw the patient. His features had undergone great change, and fever was set up. A congenital, elongated, voluminous inguinal hernia was found to exist, and M. Demarquay paid the more attention to other measures inasmuch as he had never succeeded in curing this description of hernia by operation. He applied carefully the taxis, while the patient was put into a deep sleep, with no effect, and he determined to try the effect of removing the intestinal liquids and gases by means of aspiration. A fine trocar was passed into the centre of the tumour, and, by means of Potain's aspirator, about 120 grammes of intestinal liquid were drawn into the recipient. The tumour subsided completely, and, the trocar having been removed, some minutes were allowed to elapse without touching the tumour, in order to observe whether new liquids or gases would enter the strangulated intestine. No renewal of the tumefaction took place, and very slight pressure upwards sufficed to procure the return of the intestine into the cavity of the abdomen. The patient was kept quiet, and on a low diet, fractional doses of opium being administered. No ill consequences followed. The case M. Demarquay regards as striking, and he proposes to apply this new mode of treatment—1. In all congenital hernias and in recent hernias which become strangulated at the time of their formation. 2. To old hernias which were quite reducible a few days prior to strangulation, and in large umbilical hernias that have been recently strangulated. 3. Aspiration, which has for its object facilitating the employment of taxis, should only be employed at an early period, when one can be well-nigh certain of returning into the abdomen the intestine in an unaltered state, and capable of resuming its functions.—*Med. Times and Gaz.*, June 1, 1872.

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42. *Healing of Wounds and Ulcers by a New Method.*—Dr. James Braithwaite, of Leeds, states (*Brit. Med. Journ.*, June 1, 1872), that "the application of an aqueous solution of carbolic acid (one drachm to eight ounces of

water) to an ill-conditioned ulcer, cleans it from all purulent matter, and causes it to assume a healthy red appearance, with each granulation distinctly visible. If a wound in this state be freely exposed to warm dry air for some hours, it becomes glazed and dried on the surface. It becomes covered by what is practically an impervious transparent membrane, closely applied to the surface of the granulations, and exercising a certain amount of mechanical pressure upon them. In many cases, no matter forms underneath this membrane, and cicatrization goes on underneath it with great rapidity. In time, the membrane assumes the appearance of a thin dry scab, and drops off. If matter form under the membrane, it is at once visible through it; the lotion should then be reapplied, and the wound dried by exposure, as before. Immediately this is done, the inflamed edges of the ulcer commence to pale in colour, and in twenty-four to thirty-four hours have nearly the tint of natural skin.

"This treatment is especially of value in the cure of ulcers on the leg, as no confinement to bed is necessary; and what is required in the application of the lotion and subsequent drying can be done in the evening, after the conclusion of the day's work."

Dr. B. has tried this plan in numerous cases of small wounds, but does not think it applicable for sores of large size—say six or seven inches across—from a scald.

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43. *Spinal Atrophy after Amputation.*—VULPIAN confirms by further researches his results in 1868 (*Archives de Physiologie*), and those of Dr. Dickinson subsequently published, demonstrating that, after the partial or total amputation of a limb, the part of the spinal cord furnishing nerves to the limb undergoes simple atrophy in the posterior cornu and column. These changes are more rapid and extensive, as might be expected, in young and growing subjects, than in fully developed adults.—*Brit. Med. Journ.*, May 18, 1872.

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44. *Badly-united Fracture of Femur: Refracture-cure.*—Dr. J. FAYRER records (*Indian Medical Gazette*, March 1, 1872) two very interesting cases of this.

Case I. F. M., æt. 14, healthy English sailor-boy, admitted into Medical College Hospital, on 30th November, 1871, with badly-united fracture of the left femur, causing great distortion of the limb; the thigh being fixed at an obtuse outward angle, and much shortened.

It appears that he fell from a height of about eight feet on to the deck, when the ship was off the Cape of Good Hope, about six weeks ago; and apparently had sustained a double fracture, one in the middle of the shaft of the bone, and the other in the lower third. There was much thickening from callus, the union firm and the curvature of the thigh outwards excessive, with shortening of four inches of the limb. He was put under the influence of chloroform, and the bone forcibly straightened; during the pressure, necessarily exerted for this purpose, the bone was felt to give way, as if partially fractured and partially extended.

After the operation, the limb, being restored to its normal length, was placed in a long, straight splint. Rapid consolidation took place, without a single unfavourable symptom. He was able to walk with a stick in twelve days, and was discharged, cured, on the 21st December. The callus was rapidly being absorbed, and the strength of the limb daily increasing.

Case II. Coonj Beharry, a healthy Hindoo boy, aged 10, was admitted on 22d December, 1871, with a badly-united fracture of the right femur in its upper third, caused by falling from a height of a few feet, about a month ago; union had taken place, and the limb was much shortened and distorted at an obtuse outward angle.

He was placed under the influence of chloroform, and the limb forcibly straightened by pressure over the seat of union. The bone was heard to give way with an audible snap in the site of the former fracture: the limb was then extended and placed on the long thigh splint. Slight fever followed the operation, but it soon passed away; he had no unfavourable symptom, and union rapidly took place.

On the 4th of January he was able to stand and bear the weight on the leg. He now, 18th January, walks freely on it, and is about to be discharged. There is still thickening about the seat of union, but it is rapidly disappearing; the legs are equal in length.

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45. *Bilateral Ankylosis of Lower Jaw: Operation.*—Dr. Maas, of Breslau, relates in the *Archiv für Klin. Chirurg.* (Band xiii. Heft 3) the case of a man aged 27, who was admitted into hospital with ankylosis of the jaw on both sides. It had come on after an attack of scarlet fever when he was seven years old, being preceded by severe pain in the part; and since the age of ten he had not been able to move the jaw at all. The secondary dentition was attended with great difficulty in the removal of the milk-teeth; and the new teeth were irregularly developed, and for the most part were displaced laterally. The patient, on admission, was of anæmic appearance, though in moderately good condition; the lower jaw was imperfectly developed. Speech was somewhat muffled, but was quite intelligible. Not the least movement of the jaw could be produced under anæsthesia. Herr Middeldorpf operated on the right side, removing a wedge-shaped piece of bone, as recommended by Esmarch, near the angle. The result of this was the formation of a false joint, with power of opening the mouth passively to the extent of about an inch. Between four and five months later, Dr. Fischer performed a similar operation on the left side; four months after this, the patient could voluntarily open his mouth, without pain, to the extent of about an inch and a quarter, and his general condition was much improved.—*Brit. Med. Journ.*, June 1, 1872.

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46. *Acute Inflammation of the Lining Membrane of the Mastoid Cells; Suppuration; Perforation of the Mastoid Wall; Recovery.*—Mr. John Wilkins records (*The Practitioner*, March, 1872) two cases of this, which, as the author observes, "are interesting in many particulars; acute inflammation of the lining membrane of the mastoid cells not being a very common occurrence at any time of life, and less often seen in an adult subject where there never has existed a predisposition to ear affection; and when it does occur, the end is more often fatal than otherwise; and even in recovery, the parts have become so disorganized from destruction of bone, loss of hearing, etc., that the sufferers seldom get over for life the inconveniences arising from the attack. As a rule, it is said to be very difficult to diagnose between acute inflammation of the tympanum and that occurring in the mastoid cells; but if we possess a proper acquaintance with the anatomy of the parts, we shall see that the distinction is very marked. One great characteristic symptom of acute inflammation of the mastoid cells is the severe and continuous pain experienced at the base of the occipital region; next, we have the excruciating pain felt on slight pressure over the mastoid itself, and the tumefied condition of the integuments covering that process. There, internally, the pain is deeper-seated and comes on more in paroxysms, and there is considerable giddiness; in the worst cases drowsiness and coma. On examination of the meatus, its posterior wall, corresponding to the front wall of the mastoid cells, is peculiarly sensitive to the merest pressure; when these marked symptoms are present, and the patient gets no substantial relief from a free opening in the membrana tympani, and one through the integuments of the mastoid process to the bone, we may fairly conclude that disease exists in the cells of the latter; and, knowing that acute inflammation cannot exist long in any part without suppuration, and that pus will not remain boxed up, but will make a passage for its escape somewhere, it is only rational treatment and assisting nature to open the mastoid, immediately over the seat of the disease, to allow the escape of the matter. Though pus almost invariably has a tendency outwards, if you examine the specimen of bone I have brought here this evening, you will notice it is not unlikely that before the matter could make a passage for itself through the thick and stone-like hardness of the outer wall of the mastoid process, destruction of its inner thin plate would happen, and of course speedy death would be likely to follow. Death might also be brought about by the absorption into the system of this

fetid matter by the veins, or from sympathetic brain affection, producing abscess, etc.

The happy result of the two cases here most undeniably justified the means. I had never resorted to this operation before, and I see, on referring to Toynbee on Diseases of the Ear, that he had never performed the operation, though he had spoken favourably of the probable success which might follow it. In my two cases I feel satisfied that the operation saved the lives of both patients. To perforate the bone a strong scalpel is necessary; otherwise the knife is sure to break, as mine did in case No. 1. The inflammation has a softening effect upon the outer hard table of the process, and the bone cuts as if you were forcing an instrument through a thickish piece of India-rubber. Steady and firm pressure must be made with the instrument used, in case a sudden and unexpected jerk should force it through the thin and inner boundary wall in close proximity to the lateral sinus.

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47. *Tracheotomy performed by the Galvano-caustic knife.*—M. VERNEUIL communicated to the Academy of Medicine, April 23, 1872, a case in which he had successfully operated by this method. Not more than forty or fifty drops of blood were lost. M. V. maintains that the galvano-cautery is less complicated than the écraseur; that it is very easily and conveniently managed; that the production and maintenance of the heat is readily accomplished; and finally that no process more effectually guards against the danger of hemorrhage.—*L'Movement Médicale*, April 28, 1872, and *L'Union Médicale*, April 25.

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48. *New Process for Extracting Fragments of Urinary Calculi.*—M. RELIQUET communicated to the "Société Medico-Pratique de Paris," his process for extracting fragments of urinary calculi entangled in the eyes of the evacuating sound after lithotrity. It consists in injecting continuously and forcibly through the sound, a current of water while the sound is being withdrawn. This current of water fixes the fragments against the posterior border of the eyes, and then while the instrument is being withdrawn, it surrounds the prominent portions of the fragments, and at the same time distends the parietes of the urethra. By this means few fragments are left in the bladder after every operation of lithotrity, and the number of these operations required is lessened; and further, the chance of fragments being arrested in the urethra is diminished.—*L'Union Médicale*, May 23, 1872.

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49. *Injection of Solution of Ergotine for Varicose Veins of the Lower Extremity.*—Dr. PAUL VOGT, Assistant Surgeon in the Out-Patient Clinic at Griefswald, encouraged by the benefit which Langenbeck and others have derived from treating small aneurisms by hypodermic injection of the solution of ergotine, determined upon trying it in varicose veins of the lower extremity. Hitherto, as the various attempts at obtaining a radical cure of this affection by producing obliteration of the veins over a certain extent has led to evils and dangers out of proportion to the affection itself, he has confined himself to palliative treatment. He refers (*Berlin, Kl. Woch.*, March 4) to a case in which an extensive varix which had occupied the leg for years yielded in the course of a week to two ergot injections (ext. sec. cornut. two parts, to sp. vini and glycerine of each seven parts), this being replaced by a hard, circumscribed infiltration, which eventually subsided. In various other cases as remarkable results have followed.

In explanation of the action of the ergot, Dr. Vogt believes that the following points are worthy of consideration: 1. From clinical and experimental observation the ergot injection produces contraction of the muscular coats of the vessels—chiefly of the arteries. Through the contraction of the calibre of the arteries thus produced (especially operative on those of medium calibre which are rich in smooth muscular fibre), less blood is carried into the veins, this lesser quantity being propelled with greater velocity. 2. The ergot also acts upon the muscular coat of the veins; for although this after years of persistent dilatation may have disappeared in many places, yet in others it is always still present. When, indeed, the varices have not been of prolonged

duration, that a considerable elasticity of the walls of the veins persists is shown in puerperal women, who often prior to delivery have varicose veins as thick as the finger, which only a few days after that event entirely subside. 3. It is possible that some effect may be exerted by the direct compression produced on the varix by the infiltration-swelling which results.

Whether the favourable results which in all these cases have been so striking will prove definitive cannot be decided, as sufficient time has not elapsed to allow of the cure being regarded as radical. At all events, so simple and harmless a method is well worthy of further trials; and Dr. Vogt has derived good results from its employment in other forms of phlebectasis, in varicocele, hæmorrhoids, and certain forms of angioma.—*Med. Times and Gaz.* May 11th, 1872.

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50. *M. Alphonse Guérin's Cotton-wadding Dressing for Wounds.*—This method, which was described in our No. for October, 1871, p. 561, seems to have met with much favour in France. M. Flaux, in an interesting article in the *Gazette des Hôpitaux*, Nos. 22 and 25, (*Med. Times and Gazette*, April 6, 1872) calls attention to its eminent applicability in military surgery. The experience at the St. Louis during the War of the Commune, carried on under very unfavourable circumstances, exhibited its great superiority in a remarkable manner. Success attended this treatment of the most complicated as well as the most simple wounds, prevention of purulent infection by the filtering action of the wadding being a chief cause of this. The simplicity, facility, and rapidity of this mode of dressing constitute its chief feature, several sheets of wadding and five or six strong rollers being all the apparatus required. The dressing is always applied or renewed elsewhere than in the impure air of the wards. After having washed the stump or wound with tepid water, to which carbolic acid or camphorated spirit has been added, M. Guérin cuts all the ligatures save the principal one close, and fills up the depths with small masses of cotton level with the surrounding parts. Wadding is then applied in sheets or rollers until the part is surrounded by a thick and voluminous cylindrical cuirass, which extends far beyond the limits of the wound. The limb and applied wadding being held by the assistants, the surgeon next applies a strong resisting bandage, twelve metres in length, in such a manner that after securing the proper adjustment of the wadding by its aid, he employs all the force he is master of in producing compression. However energetic or laborious this may be (never executing it with violence or jerks, however), the elasticity of the wadding renders it painless to the patient, and its being so is the test that a sufficiency of wadding has been applied. M. Guérin regards this "elastic compression" as one of the best elements in the treatment, preventing, as it does, abscess, purulent sinuses, induration, œdema, and other accidents. Under this compression, and in spite of the mass of wadding and bandage which surrounds it, the skin in the vicinity of the wound retains all its elasticity, suppleness, and physiological properties. The dressing once applied, whatever may be the seat, extent, or gravity of the wound, *all pain is immediately suppressed;* and thus, protected by so substantial a covering, the patient can be carried or walk back to the ward without any inconvenience.

Another peculiarity of this mode of dressing is the rarity of its renewal, this only taking place at periods varying from twenty-five to forty days. At the end of two or three days there is usually some loosening of the bandage, which has to be remedied by the application of a new one, this usually rendering the whole solid enough until the time arrives for its removal. When any of the secretion oozes through the bandage, or air gains admission at its upper part, additional layers of wadding and a new bandage have to be applied. This is a rare occurrence, and is usually due to insufficient wadding having been employed, the bandage not being tight enough, or the meddling of the patient. The dressing is never removed before the twentieth day, and M. Guérin has always regretted when he has yielded to the impatience of the patient, only to find the wound proceeding rapidly towards a cicatrization that ought not to have been interrupted. No fetid odour attends the non-renewal of the dressing, and the slight smell of "old linen" which is sometimes present yields to the

application of camphor or carbolic acid water. The removal of the dressing is performed out of the ward, and is speedily accomplished until arriving near the wound itself. Here gentleness is required, owing to a more or less firm adhesion that has become established between the cotton and the skin. Abundant washing in tepid water, to which camphorated spirit has been added, may then be required, or even immersion of the part. When the wound is exposed it is found to be granulating, the process of healing having been more rapid than under ordinary dressing. The amount of almost inodorous pus is usually small, even in extensive wounds, while in those of little extent it is usually absent. There are usually no traces to be found of the ligatures.

"Such," says M. Flaux, "is the dressing of M. A. Guérin. Of simple and rapid application, clean, solid, suppressing pain, and convenient for the patient —whose movements are by it rendered easy and without inconvenience—light, inodorous, and cheap, it requires after its application only intelligent surveillance unattended with fatigue. Then there is its seldomness, thus saving at the same time fatigue and suffering to the patient as well as to the surgeon, who, as in the late events, may often be overworked and at last exhausted by his labours. Finally, it is a shelter from purulent infection and the perils resulting from the overcrowding and vitiated air of a hospital. What other mode of dressing will allow of our no longer fearing this overcrowding so sadly inevitable in hospitals as they are now constructed? What other dressing would allow of our transporting to a distance with more facility and less anxiety and danger patients suffering from dangerous wounds?" He goes on to show how such advantages, great as they are in civil practice, are of tenfold importance in military surgery.

OPHTHALMOLOGY.

51. *Anæmia of the Optic Nerves treated by Galvanism.*—Mr. R. J. PYE-SMITH reports (*Brit. Med. Journ.*, May 18th, 1872) several cases of anæmia of the optic nerves (white optic disc) in males treated in Guy's Hospital, at the suggestion of Mr. Bader, to test the therapeutic value of galvanism in this affection.

"A battery of Daniell's elements was used, the electrodes terminating in small sponges soaked in warm water. The negative electrode being placed on the skin behind the ear, the positive electrode was applied to the closed eyelids of the same side, or to the brow. On closing the circuit with ten elements, the healthy eye perceived a round central illumination, generally described as about the size of a sixpence, or of the tip of the finger, irregularly circular, and of a blue tint. The impression lasted about five seconds, and then gradually faded away. On interrupting the circuit, a momentary flash was perceived over the entire field.[1] These effects were produced when the eyes were open, though more marked when they were shut. The modifications observed in the

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[1] In my own case, the defined illumination was placed somewhat to the temporal side of the field, and was accompanied by a less distinct and somewhat larger illumination in the centre of the field. These may perhaps represent the optic disc (well defined and to the temporal side) and the region of the yellow spot (central). That they are not due to the form of the electrodes, is shown by the fact that small V-shaped electrodes produced the same circular illumination. It may be mentioned that this illumination was not perceived by patients whose eyes had been excised; though one of three such patients experimented upon did perceive the flash over the entire field on interrupting the circuit, and all three of them perceived distinctly the flash over the entire field produced in healthy eyes on interrupting the circuit when passed in the opposite direction (*i. e.*, the negative electrode being on the eyelids). In one of these cases the eye had been completely excised seven years before.

patients with white optic discs will be mentioned in the account of each case. The number of elements employed was determined by the flashes perceived, the smallest number being used that produced in each case any illumination, and where no flashes were perceived an unbearable amount of burning at the electrodes, or the occurrence of giddiness, served to limit the number.

"Ophthalmoscopic examination during the passage of the current showed in all cases the veins of the retina to be congested after the first second (during which they sometimes appeared to be partially emptied), and to remain so till the interruption of the circuit, when they gradually resumed their natural fulness. This effect was observed even when no flash was perceived by the patient.

"The patients were galvanized every Tuesday, Thursday, and Saturday. The current was usually passed six times at each sitting, for eight or ten seconds each time, with intermissions of an equal period."

The following table exhibits an analysis of the cases:—

Case.	Duration of amblyopia.	Eye galvanized.	No. of elements employed.	Description of flashes perceived.	No. of days galvanized.	Result.	Remarks.
1.	2 to 5 yrs.	Both.	20	Momentary flashes on closing and on interrupting.	23	Considerable improvement.	
2.	2 to 3 yrs.	Both.	15	Momentary flash on interrupting.	18	Doubtful improvement.	
3.	2 to 3 yrs. (still slight.)	Left.	10	Normal illumination and flash.	11	Sight impaired.	Case Complicated by results of syphilitic inflammation, and (?) of Bright's disease.
4.	½ to 2½ yrs. (still slight in right eye).	Both.	15	Right normal illumination. Left none.	20	Doubtful improvement.	
5.	2 to 3 yrs.	Both.	15	None.	5	None.	Treatment not continued long enough to obtain any result.
6.	½ to 1½ yrs.	Both.	15	Momentary flashes on interrupting, and sometimes on closing.	17	Slight, but decided improvement.	Case complicated by results of syphilitic inflammation.

It is stated in a postscript that subsequently no further marked improvement was observed. To try the effect of reversing the current, Mr. Pye-Smith applied the galvanism for six weeks longer in the same way to the right eye of each patient, and in the reversed direction to the left eye. The flashes thus occasioned varied as previously stated, but no further effect seemed to be produced.

Mr. Kiddes, who tried the same treatment in the female patients, reports that the result, on the whole, was entirely unsatisfactory. "Five patients were selected, whose ages varied from 21 to 36 years. At times there appeared to be some improvement of sight, but this was soon followed by a relapse; and I attribute it more to the general health of the patient than to any benefit derived from galvanism. I have also treated six males in a similar manner (aged between 43 and 50), in addition to those reported by Mr. Pye-Smith, and no improvement of sight has been obtained in any case."

52. *Strychnine-Injection in Traumatic Amaurosis.*—In the case of an artilleryman who had been rendered amaurotic in the left eye by a blow from a splinter of shell, Dr. Werner, of Sangerhausen (*Berlin, Klin. Wochenschr.*, May 6th) injected subcutaneously 2 *milligrammes* (.03 grain) of nitrate of strychnia. The result was to give the patient slight perception of light, which had been totally absent. Convulsive twitchings, however, set in half a minute after the injection, and lasted five minutes; and the patient had frontal headache for a day afterwards. Two days after the first injection, four-fifths of a *milligramme* (.012 grain) was injected; the patient was enabled to perceive

large objects at a distance of two inches; and, on the dose being repeated two days later, he was able in half an hour to see distant objects. The last two injections were followed by slight twitchings, but no headache. The pupil, which had been in a dilated state, regained its contractility to some extent; but, when he was dismissed at the end of three weeks, there was still slight mydriasis. Ophthalmoscopic examination before the injection had shown that there was no structural injury of the interior of the eye. A similar case was recorded by Dr. Nägel, of Tübingen, in the *Klinische Wochenschr.* for 1871 (No. 9.)

MIDWIFERY AND GYNÆCOLOGY.

53. *Formidable Pulmonary Symptoms Caused by Pregnancy in Cases of Disease of the Heart.*—M. MICHEL PETER, in an interesting article (*L'Union Médicale*, Feb. 27, and March 5, 1872) points out the imminent peril which women affected with heart disease incur when they become pregnant.

Pregnancy necessarily augments the mass of blood to be circulated and consequently increases the labour of the heart, and this is most injurious to a diseased heart. A not less certain effect of pregnancy, consecutive to the first, is the production of hypertrophy of the heart, which precipitates the occurrence of the phenomena of the second stage of affections of the heart (disturbance of hematosis) and hastens the occurrence of the third stage (derangement of hematopoiesis). The practical lesson taught by this is, that pulmonary affections may be expected in women labouring under heart disease, who unfortunately become pregnant, and that precautionary measures should be adopted and active treatment resorted to in case of the occurrence of pulmonary disease.

Another practical lesson is that women with heart disease should be cautioned against becoming pregnant. A third lesson is that, after delivery, the nursing by the mother of her infant should be discountenanced, for the diseased heart, already overworked by pregnancy, ought not to have the further labour imposed upon it of maintaining the adventitious circulation of the lacteal secretion.

Further, should rapidly threatening pulmonary disease occur during pregnancy advanced to the fifth month, by ausculting the heart it is probable that a latent lesion, previously unsuspected, of this organ, will be detected, and which is the formidable cause of the pulmonary affection.

54. *Recurrent Discharge of Fluid from the Uterus during Pregnancy.*—Dr. D. DYCE BROWN records (*Brit. Med. Journ.*, May 25, 1872) an interesting case of this in a lady, pregnant about four months, who after rather more exertion in walking than usual, was wakened in the middle of the night from a sound sleep by feeling a gush of fluid from the vagina, which, on inspection, seemed the liquor amnii just tinged with blood. After this gush, there was nothing more in the way of discharge that night, and only a slight feeling of uneasiness in the uterine region. Dr. B. kept her in bed by way of precaution, but nothing further occurred till that day week, when the same discharge returned, and in the same manner. After another week, it again returned, as before, and with the same reddish tinge. Before each discharge there was a feeling of fulness and distension in the uterine region, followed by a feeling of uneasiness, which soon disappeared. The movements of the fœtus were now distinctly felt; the os uteri was slightly open. After this time, the discharge came on every two, three, or four days, and increasing in quantity. When she was about five months advanced, along with the usual discharge of clear fluid, there came an alarming quantity of blood in the middle of the night. The blood likewise came in a gush; so that after what might be supposed to have lodged in the vagina had come away, there was no further hemorrhage. Nothing else was felt by the patient than the usual temporary uneasiness. For the next month the watery discharge came nearly every night, in such quantity as frequently to soak a couple of towels. At the time of hemorrhage, there was no alteration in the os, and no evidence of partial attachment of the placenta on the os. Exactly one month after the former hemorrhage, her pulse became quick and face flushed,

and Dr. B. predicted either a return of hemorrhage or a miscarriage, but soon afterwards labour came on and she was delivered of a six months' fœtus. The amniotic sac was felt, as usual, full and tense, requiring rupture, after which a large quantity of amniotic fluid escaped. During all this time Dr. B. kept his patient in bed. The fluid in this case could not have been amniotic, as, from its continuance and profuseness, labour must have come on sooner, while the presence of the sac entire at labour requiring to be ruptured proved it.

At the next pregnancy, at about the corresponding period of advance in gestation, the same fluid again came away in the middle of the night in a gush. Having found on the former occasion that no effect in warding off the miscarriage was produced by keeping the patient in bed, Dr. B. allowed her to rise, but requested her to keep the sofa for a week. At the end of this period, the discharge occurred as before. She then rose and went about as usual, but there was no further return of the discharge, and she went safely to term. She is at present pregnant a third time, and a third time has the discharge come on at the same period of pregnancy as at first. This time, the patient would not even keep the sofa for a day, and there has been no return of the discharge. This is more than four months ago. There had been no exertion or fatigue to account for the last discharge.

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55. *New Use of Barnes' Dilators.*—Mr. H. M. Morgan records (*Brit. Med. Journ.*, May 18th) two cases in which he resorted to Barnes' Dilators with advantage.

The first was a case in which the waters broke early, and the os small and unyielding. Mr. M. thought that by making an artificial bag of waters he would materially assist labour, so with some difficulty he managed to introduce Barnes' largest bag within the os by the help of an uterine sound. When once it was there, it was easy to pump in nearly a pint of warm water; and the labour then progressed very well, each pain dilating the os by means of the artificial bag quite in a natural way. He thinks that by this means he gained some hours.

The second case was one of miscarriage at the eighth month with profuse hemorrhage, placenta prævia, and unyielding os. Mr. M. ruptured the membranes with a stiletto; and then, by means of a long pair of ovum-forceps, he passed Barnes' largest bag (rolled up small) quite into the womb; and afterwards pumped into the bag nearly a pint of warm water. As she had no pain worth mentioning, he had given her forty minims of liquid extract of ergot before puncturing the membranes. As soon as he had filled the bag with water, he commenced dilating the os himself by drawing at the tube of the bag till his finger and thumb could reach the root of the tube in the vagina. The result was, that in little more than five minutes, he pulled a soft bag, as large as an ordinary new-born child's head, through the os; and then the vagina and perineum were gradually dilated in the same way until the bag came quite away. He found the head presenting, and pains were coming on moderately; but, as there was no time to be lost, he preferred not to wait for nature to act; so, with one hand internally, and the other externally, he turned the child and brought a foot down, and soon completed the labour with the aid of the patient's own pains and efforts. The placenta was expelled naturally, and the womb contracted well after it. Dilatation, he says, would not possibly have been accomplished so quickly, so easily, so painlessly, and so safely, by the hand. Moreover, this bag, by being pressed against the bleeding placental vessels in its passage through the os, compressed them and checked the hemorrhage, in the same way as the head does in those cases where the pains are strong enough to keep it well pressed against the os.

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56. *Utero-Peritoneal Fistula.*—A remarkable case of this is recorded (*Lancet*, May 18, 1872) by Mr. Lawson Tait. The subject of it was aged 24, was married at eighteen and had five children, the last six weeks before seen by Mr. T. (March 4th). She said her last labour was prolonged; she lost a great deal of blood and the hemorrhage has never since ceased. She was anæmic and had a worn-out appearance. On the 8th of March Mr. T. introduced a sound into the uterine cavity three inches; "but after reaching this distance the sound

slipped, without the exercise of any force, through the fundus, apparently about midway between the cornua and somewhat on the anterior aspect, and could readily be felt under the abdominal parietes. The woman is so thin that the position of the exit of the sound can readily be felt. On this day I passed the sound twice, and each time found that it only required a little manœuvring to pass it at the one spot." Mr. T. passed the sound subsequently in the presence of several physicians and satisfied all as to the fact. This fistula Mr. T. believes had its origin in a rupture of the uterus during labour, remaining undiscovered until the walls of the uterus became thinned by the process of involution.

"Cases of perforation of the fundus by the sound." Mr. T. says, "are, I am convinced, by no means so rare as one would be inclined to believe, for I have seen at least five or six, and Sir James Simpson used to speak to us of having seen many, They are very curious, and give rise to many interesting subjects for consideration. For instance, is it likely that the subject of this fistula will ever again become pregnant? I see no reason why she should not, and I believe the fistula will remain patent until she does."

57. *Inter-menstrual or Intermediate Dysmenorrhœa.*—Dr. W. O. PRIESTLEY, in a paper read before the Royal Med.-Chir. Society (Nov. 28th), pointed out that, although much had been written concerning dysmenorrhœa, and several forms of it had been described in accordance with the pathological views taken of its causes, the description of the several varieties was ordinarily limited to the time of the catamenial period, with the two or three days additional which may precede and follow the menstrual flow. From time to time, however, cases of a more obscure kind presented themselves, in which the chief suffering is remote from the actual menstrual period, but comes on, nevertheless, with the same punctuality, and is probably dependent on organic changes associated with the production of the catamenia. Probably other practitioners had observed like instances, as they were not unfrequent, but as the author had met with no description of them, he brought the subject before the Society as a fragmentary contribution to the pathology of uterine affections, which might possibly evoke further elucidation by discussion.

In all the cases detailed severe pain was experienced by the patients midway in the menstrual interval. The pain commonly came on about fourteen days after a catamenial period, and, after lasting a variable number of days, ceased before the supervention of the next expected period. In one case, the pain, beginning midway in the interval, ran into the following monthly period, and was relieved by its flow. The suffering was constantly referred to one or other ovarian region, and in three cases out of four marked tumour, or thickening from old adhesions, was found in that locality.

The reason for the occurrence of pain in the inter-menstrual period, and with such regularity, was not, in the present condition of our knowledge, perfectly obvious. A study of the physiological and pathological conditions left little doubt, however, that it was due to perturbations in the function of "spontaneous ovulation" habitually going on in the ovary. Hypertrophy of the structure of the ovary, or thickening of its indusium, would lead to undue vascular excitement, and impede the advance of ova to the surface in their attempts to attain maturity. It was not unreasonable to suppose, from all the known facts of the case, that preparation for an approaching period began in the ovary ten or fourteen days before the occurrence of the monthly uterine discharge, and if the initial steps in the process of ovulation were opposed by certain pathological conditions, pain would ensue. Nay, in the absence of distinct organic change, it might readily be imagined how special irritability in the ovary would cause an unusual amount of disturbance whenever there was occasion for the exercise of fresh activity in the organ. This latter class of cases would partake more or less of a neuralgic character.

The treatment would depend on the pathological condition as ascertained by examination. The pain being only a symptom, it would be needful to inquire into the cause; and if there were tumour, or thickening depending on former inflammation, absorbent remedies would be indicated. If no organic change of structure could be detected, anti-neuralgic remedies, such as quinia, iron, and arsenic, would best answer the purpose of cure.—*Lancet*, Dec. 9, 1871.

CORRESPONDENCE.

Contributions to the Pathology of the Blood. By Prof. Von S. Stricker. Translated from the German by F. J. Bumstead, M.D.

[In the American Journal of the Medical Sciences for January, 1868, Prof. J. H. Salisbury, of Cleveland, Ohio, published a "Description of two new algoid vegetations, one of which appears to be the specific cause of syphilis, and the other of gonorrhœa."

The subject attracted but little attention until on the 26th of January, 1872, when Dr. Adolph Lostorfer (see *Archiv. für Dermatologie und Syphilis*, IV. Jahrgang, erstes Heft, 1872), read a paper before the Wiener Gesellschaft der Aerzte, a paper, "*Ueber die specifische Unterscheidbarkeit des Blutes Syphilitischer*," which, for the moment, excited on the part of many of the members a furore of applause, but which met with much subsequent opposition and gave rise to discussion of a violent and personal character.

Prof. Stricker, whose pupil Dr. Lostorfer was, then undertook a series of investigations upon the subject, at which he kindly invited me to be present. The results are given in the following pages, which I have translated from advance sheets of the *Medicinische Jahrbücher*, furnished for this purpose by Prof. Stricker. It is not inappropriate that they should find a place in the Journal which first published Prof. Salisbury's paper, although no one, I presume, after reading the two papers, will be inclined to believe that Prof. Salisbury's "crypta" and Dr. Lostorfer's "corpuscles" are one and the same thing, and Prof. Stricker does not regard them as identical.

But however this may be, it is fortunate that the fungous origin of syphilis has been so thoroughly investigated by such a keen and able observer as Prof. Stricker, and so far as the physical elements of syphilitic blood are concerned, it is safe to say that they are here described as fully and as perfectly as the best microscopes of the present day will enable us to appreciate them. F. J. B., Vienna, May 12, 1872.]

I undertook the following investigation to decide two questions; I desired in the first place to ascertain the nature of the corpuscles discovered by Lostorfer, and in the second to know, from my own examination, whether these corpuscles, as Lostorfer had asserted, were peculiar to the blood of syphilitic persons.

I commenced my investigations with the blood of three syphilitic men, and in all three the corpuscles in question were found in abundance in the course of a few days; I therefore supposed that I had obtained abundant material for the solution of the first question.

A preliminary inquiry was, whether Lostorfer's corpuscles are not present in fresh blood. Before answering this, I will describe the mode in which I prepared and kept my preparations.

In preparing specimens of blood merely for examination in a fresh state with the highest powers of the microscope, I endeavour to make them as thin as possible. The slide and cover glass should first be carefully cleansed and dried. A small puncture is made in the integument of the person whose blood is to be examined, from which the blood will ooze forth. The covering glass is now held by its parallel edges between the thumb and forefinger, and its central portion is brought in contact with the apex of the drop of blood emerging from the wound, so that a minute portion adheres to the surface of the glass. If the covering glass is now quickly applied to the slide, the minute drop of blood will, in most cases, spread itself out sufficiently; otherwise, a gentle pressure

upon the covering glass is required. Since the examination is to be carried on with immersion lenses, and since it is of importance to be able to examine the specimen near to its edges, while, on the other hand, any overflow of water beyond the sides of the covering glass spoils the specimen, it may not be superfluous to remark that I apply the water, required by the immersion lens, only in very small quantity and to the upper surface of the covering glass by means of a pipette, drawn out at both ends to a very fine point.

The appearances to be seen in fresh blood are manifold. In the blood of many persons, to be sure, only the well-known elements are found, to wit: red blood corpuscles, colourless blood cells and smaller, colourless nucleoli. Very frequently, however, there appear in larger or smaller numbers colourless bodies from the size of a nucleolus to that of a colourless blood corpuscle. These are especially visible when working with very high powers.

It would be fruitless to describe the varied forms of these bodies. I will only say that many of them appear as if they were fragments of young cells, whilst others give the impression of being knotted points of coagulation. The latter are chiefly found in places where the preparation is somewhat thick, and where the interspaces between the blood corpuscles are traversed by numerous fine threads.

It is not improbable, therefore, that the branched, knotted points, as well as the fine threads, consist of fibrine. Those bodies which look like portions of colourless blood corpuscles are, like the last mentioned, various in form and seldom spherical.

Wherever a rich net of fibrine is present, numerous and exceedingly fine particles are to be found, but it is easy to convince one's self that they are for the most part sections of fibres running parallel or obliquely to the axis of the microscope.

Besides the above mentioned constituents, very minute particles are seen in the blood of many persons, which can scarcely be defined by an immersion lens No. 10, and which are frequently in active oscillation. Whether these oscillations are exclusively Brownian, or whether the particles are sometimes organisms showing vital movement, I will not attempt to decide. The question, however, appears to me worth raising, in order to call attention to the fact, that the existence of very small organisms of a low order in the circulating blood of healthy persons is not excluded.

With regard to the presence of such foreign bodies as accidentally get mixed with the preparation, any one conversant with the microscopical examination of the blood, can decide.

In rare instances, however, corpuscles are found in fresh blood, with regard to which it is difficult to say whether they are an accidental admixture, or if they were previously present in the circulating fluid. They are, for the most part, spherical, with a dark outline, and they are distinguished by the fact that when seen under a very high power (objective 15 of Hartnack) and with sharp adjustment, they are very dark.

For the purpose of breeding the corpuscles which we now have in view, we have to preserve a good specimen of blood under exposure to the air, for as long a time as possible. This object will be attained if the preparation be kept in a chamber in which it is on the one hand protected from drying, and on the other hand from moisture. That the drying of the preparation is not advantageous, is self-evident. The same is almost equally true of any thinning of the blood through the addition of moisture. The corpuscles of the blood are very susceptible to any thinning of the plasma in which they float. The red corpuscles become pale; the plasma takes up colouring matter and becomes turbid. I will hereafter show that the gradual addition of water is highly prejudicial for

the purpose we have in view. I can only state in general terms how these two conditions, viz., preventing the specimen from drying, and also keeping out water, are to be attained. Every experimenter must determine the finer shades for himself. Place then the specimens in a chamber in which the surface of water therein contained shall be small in proportion to the size of the chamber. For an exsiccator glass, such as chemists use for drying objects, a moist sponge the size of a walnut is sufficient to protect the specimens from drying. Beneath this glass I place a framework that will hold about a dozen specimens. If the slides are labelled, specimens from various sources may be placed in the same chamber and upon the same frame.

In order to favour the development of the corpuscles, I prepare the specimens somewhat thicker than above described. Before collecting the blood, I warm the slide and smear upon it two thin strips of wax in such a manner that the covering glass can rest upon them. After the specimen is prepared I press the covering glass down upon the wax until the rolls of adherent blood corpuscles are spread out in the form of a net. The meshes (plasma-islands, or interspaces) of this net, filled with plasma, afford breeding spaces which are admirably adapted for our purpose.

It is desirable, for reasons to be given hereafter, to make the specimen, relatively to the covering glass, so small that its edges may be examined with an immersion lens.

When the specimen is thus put up, it is also easy to test microchemically the granules which are found in the plasma-islands. The preparation of blood is, in fact, transformed into a comparatively firm cake by means of the threads of fibrine which run through it; the granules themselves are often seated in a network of fibrine; one or more of these can be maintained in the field of vision, whilst the reagent is allowed to flow in in exceedingly small quantities from the edge of the covering glass. The significance of this mode of preparation will be understood, when it is recollected that in fresh and even in older preparations of blood when thin, any addition of fluid sets the constituents in motion, which is very soon perceptible under high magnifying powers. Under these circumstances, if you wish to know how a certain granule is changed by the reagent, you must endeavour to follow it by means of corresponding motion of the slide; but since the granule is not alone set in motion but the red corpuscles also swim with it, the former is soon covered over by the latter, and the eye and the hand must be taxed to the utmost to follow the object in its course. Even then the same degree of certainty can never be attained as when the body remains quiescent, while the reagent is allowed to slowly act upon it. Especially when there are a number of corpuscles in a plasma-island near the edge of the preparation, and the reagent is allowed to flow in from the same side, the reaction can be perceived as soon as the fluid reaches the blood.

In case I wish to examine for several hours consecutively any one portion of a specimen of any one granule in particular, I build up a wall around the slide upon the microscope. Said wall consists of four pieces of folded, moistened and bibulous paper, which are kept wet from time to time by means of a drop-bottle. In this way there is no trouble in keeping one preparation under the microscope for eight or ten hours.

I have obtained some interesting results by being able to keep my preparations for several hours in succession upon a heated slide. For this purpose I used the heatable object-table, invented by me and described in my *Handbuch der Gewebelehre*. To this table I attached a copper wire about twenty-five ctm. long, and, placing a small spirit lamp under the extreme end of it, gradually brought the latter nearer and nearer to the microscope, until the desired temperature was attained. I then enveloped each end of the slide with moist-

ened blotting paper, so that while the middle of the slide rested upon the metal the ends of the same found steady support upon the hard rubber by means of the paper. Two more folded strips of moistened paper were then placed uprightly on the two remaining sides of the slide, one before and the other behind the objective lens of the microscope. Twice I moistened these strips of paper from time to time; my specimen was protected from drying in spite of the artificial heat. Finally I placed a large screen before the microscope, which concealed the flame of the spirit lamp from the eye of the observer. In spite, therefore, of the loss of light occasioned by the use of the heating apparatus, I was able to work with lenses of high power.

When the number of specimens to be examined prevents us from watching any one of them continuously, as we might wish to do, it is well to have some fine shreds of silk floss, to serve as landmarks, running through the preparations. These are best taken directly from a cocoon and are to be applied to the slide before it is charged with the blood, by laying them across the field and sinking them into the two strips of wax before mentioned. By this means it is very easy to find again any given point, even when using the highest powers, especially if assisted by a sketch upon paper of the bearings made at the first examination.

By the method which I have now described I have in a large number of specimens been able to watch, for several days in succession, the elements which are to be seen in fresh blood, and I am now able to state that those bodies which I have mentioned as due probably to coagulation, or which may be torn fragments of protoplasm, disappear, as a rule, in the course of twenty-four hours. It is, therefore, for our present purpose, immaterial whether my suspicion as to their nature is correct or not; for the only question at present is whether these bodies are identical with those discovered by Lostorfer, or whether the latter are produced from the former; in either case Lostorfer's bodies would be found, especially in the fresh blood, both of sound as well as diseased persons. Such, however, is not the fact. I have, in a series of fresh specimens of blood carefully drawn and registered all that was to be seen in the plasma-islands, and watched the bodies just referred to until their contours had become indistinct and pale, when around them new bodies, not visible before, would spring up, and, on repeated examination, be seen to grow larger and larger, until they finally assumed the unquestionable characters of Lostorfer's corpuscles. In what way the bodies visible in the fresh blood disappeared I cannot explain, but such explanation does not pertain to the question in hand.

The inquiry which I raised at the outset whether Lostorfer's corpuscles are to be found in fresh blood, is now partly answered, but I can relate an observation which disposes of the question still more decisively.

In Prof. Zeissl's syphilitic wards, in the month of March of this year, was a man whose blood was unusually favorable for my investigation. The patient was twenty-two years old, somewhat poorly nourished, and had suffered with syphilis for four months. I took several drops of blood from his vola manus, prepared a number of specimens, and after twenty-four hours found the edges of the latter strewn with the looked-for corpuscles. A second set of specimens showed the same result. On trying a third set, no corpuscles were to be seen the first thirty-six hours, and were only visible at the end of forty-eight hours.

At this time I had just stopped having my room warmed, although the weather was still somewhat cool. On considering the different circumstances which might have occasioned the delay, I was led to take the temperature of the room into account. On the following day I had my room heated up to 22° Centigrade, obtained new specimens, studied them carefully, and noted down all that was to be seen in several portions near the periphery. In the course

of three hours the corpuscles had made their appearance in large numbers. Several of them were so large that I was forced to one of the following conclusions: Either, in spite of all my previous observations, I had deceived myself, the corpuscles had been present in fresh blood and had escaped my detection, or their growth in this case had been very rapid, and it must be in my power to bring it under direct observation. On the following day I took again a fresh specimen, had my chamber warmed as the day before, placed a portion of the specimen near the periphery, under Hartnack's immersion lens No. 10, and watched. About fifteen minutes had passed since the drop of blood was taken, when it seemed to me that I could detect, in the otherwise clear plasma-interspace, a few granules. In the course of ten minutes more these granules had assumed so decided a character that I could clearly define their positions. At the same time it appeared to me as if numerous fresh granules were making their appearance in the same interspace. I could not look continuously, since, to spare my eyes, I had to look up from time to time, but I was able to fix the position of certain granules so distinctly that I recognized them at once at each fresh glance. At the end of half an hour the granules which had first appeared were already so large as to enable me to recognize them as the bodies I was looking for. At the end of an hour and a half the first granules had grown to the size of a nucleus of a pus corpuscle, and the whole plasma-interspace was scattered over with similar but smaller granules. There was no longer the slightest doubt; I had before me Lostorfer's bodies; they had made their appearance and grown up in a clear plasma-interspace under my own eyes.

On the following day I repeated the experiment, but, instead of heating my room, I laid the specimen upon my heatable table, warmed up to 25° Centigrade, and repeated exactly the observations made on the previous day. The conclusions drawn from numerous earliar examinations, continued over several days, were now supported by direct observation, and my question was answered with certainty.

Let us now turn to the first and chief question. What is the nature of these bodies? They are either organisms or they are inorganic. From what we have thus far learnt we cannot decide with certainty in favor of either of these possibilities. We know that they grow, but growth alone is not sufficient.

Lostorfer has stated that the bodies bear sprouts. This statement is also true; but I have ascertained by exact observation that some of the sprouts arise not through a process of growth, but through the apposition of other bodies.

When quite fresh most of the corpuscles are spherical and under No. 10, and even No. 15, immersion lens, are seen to be homogeneous. Now it was my good fortune to observe how a small corpuscle, which had been lying for some time in the neighbourhood of a larger one, approached the latter and became attached to it like a button. After a time the contour changed in such a manner that the sharp constriction between the two bulged out and became concave on either side; then the concavities became effaced; the general outline was club-shaped; then the summit was gradually drawn in and the club was changed into a circle. I could, of course, not say that all the bodies provided with sprouts had arisen in this way, but it was enough to have once distinctly witnessed such a mode of origin. It was enough to have shown that the appearance of sprouts already formed proved nothing as to their mode of life.

Lostorfer has also stated that the corpuscles sometimes grow into sack-shaped bodies or tubes. I only once saw a corpuscle with a long, adherent tube, but in this instance I distinctly observed its mode of origin, which was due to the arrangement in a row of three small corpuscles that at the commencement of the observation lay near each other by the side of a large one.

The interspaces or constrictions became effaced, and a homogeneous tube projecting from the large corpuscle was completed. Hence, no inference is to be drawn with regard to their organization, either from the sprouts or from the branches exhibited by the corpuscles.

My observation with regard to the origin of sprouts throws much doubt on the phenomena of growth, of which I spoke above. Having once determined that the smaller corpuscles blend with the larger, why should the smaller ones not grow in consequence of the aggregation upon them of still smaller ones? It is true that the corpuscles, whose growth I witnessed, showed no signs that their growth was due to apposition, but that is nothing to the purpose.

My observations have, therefore, shown that the corpuscles appear in the field of vision at first so small that they can scarcely be perceived. Hence corpuscles must exist of an exceedingly minute size. But further, it is not improbable that they exist at the outset beyond the limits of vision, and only become visible through their growth. What prevents us also from supposing that the corpuscles, while growing before our eyes, grow by the apposition of smaller invisible bodies? It will be objected that we have no example of such growth in any non-crystalline bodies; but do we know of any instance in which an organism grows so rapidly as must be the case when, in the course of an hour, it increases from the limits of invisibility with an immersion lens No. 10 up to the size of a very large nucleus?

It therefore appears to me conclusive, that we can decide nothing as to the nature of the corpuscles from their increase of size. Thus much only is determined, that we have to do with entirely unknown structures.

Meanwhile I had observed another fact which led me somewhat nearer to a conclusion as to the nature of these bodies. I had ascertained that their chemical reaction is different immediately after their appearance upon the heatable table from what it is when they have been developed to a large size by being kept in a moist chamber for several days. In the latter case they are but little affected by acids or alkalies; they shrink a little, but are not destroyed. In an early stage of their existence, however, they are destroyed by acids and alkalies, and even by water. For this reason it is important that the specimen at the outset should be protected from water. When once grown, they will survive for days, even when the blood-corpuscles have been destroyed by moisture. This peculiarity can be readily explained under the supposition that these bodies are organisms. In that case they would have their youth and their adult life; and these would correspond to the early and the late stages of animal cells. In the former their outer layers would be relatively soft and in the latter hard.

If, however, we suppose that the bodies are not living but consist of colloid substances, the possibility that their outer layers may then also become hardened in the course of time, is not excluded.

With these doubtful conclusions, I was now on the point of terminating my investigations with regard to this question, and wanted only to try if a still higher degree of heat would not afford useful information. I had already made the attempt before with the blood of another patient, and the bodies which had gradually appeared had again disappeared through the formation of vacuoles. Now I wished at least to investigate the development of the vacuoles.

Upon warming a specimen of the blood of the above-mentioned patient very slowly and carefully up to about 38° Centigrade, I saw a remarkable sight. A perfectly clear plasma-interspace was in the course of about fifteen minutes so filled with very fine bodies, that I might have supposed that a precipitate had taken place. It was scarcely possible to watch the development of any one body. Their number was too great and they lay too near each other; after

necessarily resting my eyes for a moment I could not be certain that I was really looking at the same one as before. One thing, however, was certain, the number of the bodies was gradually increasing, and soon some of them were so large that I could decidedly recognize them as Lostorfer's corpuscles. I noticed with surprise that a large number were provided with bulbous-shaped projections. I now fixed my eye, so long as I could look continuously, upon one of these, and found that the two bodies, the larger and the smaller, soon approached each other and again rapidly became separated. After this observation, which was somewhat trying to the eyes, I was obliged to rest for a short time, and whenever I commenced again I was obliged, for reasons already given, to watch a new pair, of which, as before stated, there were many in the field. I first noticed that the two bodies always had a swinging to and fro motion, as if they were firmly bound together. By changing the light I succeeded occasionally in seeing the thin and structureless thread which bound them together. Finally, I observed most distinctly how two such bodies slowly approached each other, then again separated, once more approached, and once more separated, and finally broke suddenly apart.

The number of the granules apparently still further increased, the field of vision was thickly scattered with them, but they did not attain to a considerable size; they remained much smaller than when more slowly developed at a temperature of 22°–25° Centigrade. In spite of careful observation I did not at this time succeed in observing any union between two corpuscles; although, in many cases, I saw the bond of union between them becoming broader and shorter, so that occasionally the two appeared to be one with a broad crevice upon it. Still even this crevice became broader and narrower, and respectively longer and shorter at intervals.

After these same phenomena had been observed again on the following day in blood from the same patient and under the same conditions, I regarded the chief question as answered. So far as we know, the phenomenon of repeated approach and separation between two bodies can only be explained upon the supposition that their bond of union is contractile and that the bodies themselves are organisms.

The phenomena of growth now assumed greater precision. I have before remarked that, if we observe the running together of two fluid bodies, we may explain the growth in question as an instance of mere apposition. I only stated, however, how I explained the matter to myself, before having further observations at my disposal, and I coerced the circumstances so as to leave no room for a supposition which was open to objections; for in all my observations of the union of two of the bodies I never saw such a running together as takes place between two drops of fluid. When two drops of fluid are lying against each other they may for some time adhere to each other and present the form of a sprout; but so soon as their surfaces at any point are broken, and so soon as their masses become confluent they suddenly form one drop. In the case of the corpuscles this process went on slowly; the contours gradually changed; the form of a sprout was changed into that of a biscuit; then one incision after the other became effaced; and even then upon the upper, or upon the highest surface was a line of separation visible, until finally an oblong form was established without trace of division. In this oblong form, the bodies continued for some time longer. I saw repeatedly that facets were even at this time formed and then became effaced. The final result of all these changes was a globular form, with which the motions ceased. I was not willing to regard these slight changes of form as indications of life, because I could not observe the disappearance and reappearance of distinct forms. I was well aware, however, that these changes of form are frequently present in protracted processes of division. It was also

just as probable at least to suppose that two bodies lying near each other, and that suddenly approached each other, were bound together by a contractile thread, as that they were approximated by mutual attraction.

In the processes of division to be seen in the tissues of a frog, when portions of the same have been laid bare or have been cut out of this animal, I have often seen how two halves of one cell separated from each other until they were only united by a thin thread; how the thread sometimes changed its optical characters, until it became invisible; how the division was then apparently complete, and yet the products of division again approached each other, and gradually united. All experienced pathologists are also conversant with the fact, that pus corpuscles, so long as they are exposed to certain conditions of division, vegetate in such a manner that they do not attain any considerable size. On the contrary, when the conditions are unfavourable for division, their growth is considerable, and they then begin to form permanent tissue. This observation corresponds in every respect with the action of our corpuscles, which remarkably increased in size at a temperature of 22–25° C., while at the temperature of 38° C. they grew less, but increased in number instead.

We have, therefore, besides the appearances observed on warming the preparations, a noticeable series of indications in favour of the supposition that the bodies are organisms; indications which are of themselves not sufficient to prove the supposition, but which fall in with it exceedingly well.

We have to do with organisms which are developed only very slowly at a temperature below 10° C., and at a decidedly lower temperature perhaps not at all; at a temperature of 20–25° C., their development is somewhat rapid, and is still more rapid at the temperature of the human body. When they have once attained a certain size, they then remain in the form of globules or discs (in many cases it was not possible for me to make the differential diagnosis), either in a state of rest, or as dead bodies; they can then be kept for days, even when the preparation of blood is destroyed by moisture.

These organisms increase by sprouts or by division. The outgrowth of large threads or filaments was not, however, observed in any place, or under any of the conditions to which I subjected them.

Their development is essentially favoured by the neighbourhood of a large number of blood-corpuscles, and again, by the neighbourhood of air bubbles. They will usually first be found where a plasma-interspace is bounded on one side by air, and on the other side by a thick layer of blood-corpuscles. When the preparations are put up in the manner above described, and are first examined over their entire margin, the bodies can scarcely be overlooked, if they make their appearance at all.

When they have grown under my own eyes, they have very decidedly spread from the margin, and, therefore, from the confines of the air towards the centre of the preparation. Still it is not to be understood that I have seen how the bodies advanced. I have on the contrary observed that the finest granules always begin to be visible at the same time in one zone; and first in a zone in the neighbourhood of an air bubble. In a short time, the whole of this plasma-interspace would be filled with them, and only afterwards could I perceive analogous appearances in the interspace the next farthest removed.

With regard to the sources from which these organisms proceed, we have the following alternatives. They arise either from the air or from the blood; and in the latter case their germs already existed in the circulating fluid, or they sprung up subsequently by spontaneous generation.

The last portion of the alternative will indeed find few advocates, and I myself would not be included among the number. But nobody will deny that in this case *generatio æquivoca* must come into consideration as one of the

bilities; for so soon as we have gone so far as to assert, that organisms arise from germs which are not to be seen with the best microscopes in existence, we assert something which cannot be proved, so long as the possibility of *generatio æquivoca* is not excluded with all imaginable certainty. *Generatio æquivoca* is not proved, but its possibility is not out of question.

In case the germs arise directly or indirectly from the air, we have perhaps to do with organisms which are well known under other forms, perhaps with bacteria, which finally appear in every preparation that is allowed to stand. Only we must then suppose that they assume in the blood of certain men, a mode and form of development, which, so far as I know, have never hitherto been discovered.

The fact that their development first begins where the plasma borders upon a layer of air, is perhaps in favour of their arising from the air. But it is also to be borne in mind that perhaps the air in the neighbourhood only favours their development; and this is the more worthy of consideration, since I have found that the development proceeds most favourably when the layer of blood, bounding the plasma-interspace upon the opposite side, is somewhat thick.

Let us now suppose that these newly known organisms arise neither directly nor indirectly from the air, and also not by spontaneous generation. We must here consider, that in the blood of one individual, derivatives from the cells of this individual or of other similar individuals may circulate, which the first obtained from the secretions of his parents or through contagion from one individual to another. The supposition that such derivatives form cells, which in consequence of their smallness are not visible under the best microscopes, is indeed entirely hypothetical. It must, however, be confessed that the existence of such is not at the outset to be excluded.

I turn now to the second question, viz., whether the organisms in question are present only in the blood of syphilitic persons? This question I must at the outset answer in the negative.

I have examined, in all, thirteen cases of general syphilis, and in these I found the bodies in nine cases in large number; in two cases they were certainly not to be found, and in two cases they were so few that for reasons hereafter to be given I was obliged to regard the result as negative. Among the cases which I have examined, we have, therefore, nine successful and four unsuccessful.

At the commencement of my investigation I found no trace of these bodies in ten cases of healthy, well nourished individuals, in one case of pneumonia, one of heart-disease, four cases of typhus abdominalis, one of typhus exanthematicus, and ten cases of smallpox.

The first case of lupus which I examined showed them in considerable number, and in nine more cases of lupus I found them once again; so that they were present in two out of ten cases of lupus.

After this experience, the matter, as Lostorfer had represented it, appeared to gain in probability. I had found then out of thirty-seven non-syphilitic persons only in two, and in these two cases the supposition was allowable that between lupus and syphilis there is no absolute distinction.

In the meanwhile, a circumstance occurred which turned my attention in a new direction. The patient above mentioned, whose blood afforded me an opportunity to observe the development of the corpuscles in so short a time, had several pretty severe attacks of hæmoptysis. Since I had observed the appearances which I have described in no other case so well marked as in this, the idea that deficient nutrition either alone or in combination with some other general disease, might have an essential influence in the production of the bodies, forced itself upon me. I therefore determined to examine the blood of persons suffering with severe chronic affections. My examinations were,

indeed, few in number, but they were amply sufficient to enable me to decide the question. I found the bodies in large numbers in one case of carcinoma ventriculi, and in two cases of tuberculosis, and in all three cases on the second day of keeping the specimens. Twice I also found them in moderate numbers in another case of advanced disturbance of the nutrition, in which Bright's disease was combined with disease of the heart; and again in a case of anæmia following variola, the question appeared to me to be settled so far as was desirable for the present.

The investigation of these cases justified the decision that the organisms we have described are not exclusively to be found in the blood of syphilitic persons.

When we, however, consider that I failed to find the corpuscles in the blood of a very considerable number of persons, a portion of whom were healthy and the remainder affected with acute diseases, we cannot deny that Lostorfer's discovery is of some consequence in the pathology and especially in the doctrines of syphilis.

I will not assert that these bodies are not present in the blood of sound persons, and of persons affected with acute diseases. The number of cases which I examined is much too small for such an assertion. Thus much, however, is evident from my observations, that these new elements of the blood are present —seldom in sound persons and in persons with acute diseases—but very frequently in persons suffering from long continued chronic disturbances of the nutrition, and in persons affected with syphilis. We must now wait for the results of farther investigations. Should it be proved that my observations are correct, or nearly so, when applied to a large number of cases, I may have something further to say about the facts.

In conclusion I wish to show that the possibility of a very close connection between Lostorfer's corpuscles and the appearances included under the name of syphilis, is for the present not to be rejected. This will chiefly depend upon the question, whether the germs of these bodies were or were not previously present in the blood. If the latter be true, then indeed the supposition of such a near connection will be little justified. For it will then be more natural to suppose that there exists in the blood of certain persons a chemical condition, which exerts a favourable influence for the development of organisms coming from without. Should it, however, appear that the germs exist in the blood, then we shall have reason to suppose that organisms under certain conditions may assume certain peculiarities, and also among others, those peculiarities which we ascribe to the syphilitic virus.

These considerations will also be justified if larger statistics should prove incontestibly that the organisms are really present in the blood of all men who are affected with long-continued disturbance of their nutrition. For there is nothing against the supposition that the syphilitic virus may be generated in man under conditions which are unknown to us; that long-continued disturbance of the nutrition establishes such conditions, and that the virus first assumes its peculiarities under certain conditions, perhaps when it passes in single or manifold descendence from one individual to another.

The circumstance, that these bodies were found in large number precisely in cases of syphilis, carcinoma, and tuberculosis, is surely not opposed to such an assumption.

The circumstance, that in all three diseases we meet with a series of wholly unexplained phenomena, should stimulate us to follow every track that may lead to an explanation so long as it is scientifically justified.

Finally, the circumstance, that my numerous and careful investigations of the blood of syphilitic persons were fruitless in every other direction than that stated by Lostorfer, may serve to further test his discovery until we can obtain light as to its value.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

On the supposed Action of Quinia on the Gravid Uterus. By JAMES BROWN, M.D., of Newcomerstown, Ohio.

I have spent about thirty years in the practice of medicine in the Tuscarawas valley, where miasmatic influences stamp their peculiar impress upon almost every form of disease, making quinia a prime necessity throughout the successive seasons of the year. I do not hesitate to prescribe it without reference to the *fact* or the *stage* of pregnancy. I give it not only with a view to arrest an attack of intermitting or remitting fever, or to support and sustain the powers of life, sinking under the influence of disease which I cannot cut short, but I give it to pregnant women the same as to other patients, in conditions of comparative and comfortable health, with a view to anticipate and ward off an expected *return* of ague.

In this long, extensive, and varied experience in the use of the remedy in question, I have never been led to suspect it of any power or tendency, in proper doses, to awaken uterine contractions, or in any way to disturb the normal functions of the uterine system.

Is Quinia an Oxytocic? By G. ERICKSON, M.D., of Kendallville, Ind.

My medical experience extends over a period of eighteen years, in one of the most malarial sections of northern Indiana, where, during some seasons, in my earlier practice, there were not enough well persons to take care of those suffering from malarial diseases. In those primitive times here a very large majority of the families were young, consequently the obstetric cases were much greater in proportion to the population, than at the present time. Quinia was the almost universal remedy for all malarious diseases, prescribed freely, from twenty to forty grains in intermittents and remittents, but in the latter it generally had to be repeated. In the pernicious forms of these diseases, from sixty to one hundred grains were given, usually in the course of a few hours, yet even with this quantity the remedy had usually to be repeated, though in diminished doses.

This treatment was practised in pregnant patients just as in others, and but few females escaped the fever at some period of gestation, so prevalent was it.

Yet not in a single instance did it develop uterine contractions "de novo," but if they existed at the time quinia was given, then, in a certain number of cases, it certainly did increase their force, but had but little influence on their frequency. Its effects as an oxytocic were most marked in the advanced stages of labours, especially at or near the full term of gestation, while in the earlier stages, especially in miscarriages, it seldom produced any perceptible effect upon the uterine contractions.

The severer forms of malarial diseases were not an uncommon cause of miscarriages, if allowed to continue, but even when moderate uterine con-

tractions had commenced and no hemorrhage had occurred, these contractions usually ceased if the paroxysms of the disease were promptly arrested by quinia.

Morphia prevented its oxytocic effect.

My conclusions may be briefly stated to be these: That quinia does not develop uterine contractions "de novo."

That if they exist in a moderate degree it has but little or no influence over them. But in advanced stages of labours, especially at or near full term of gestation, then its effects, in some cases, are more marked as an oxytocic, though in a minority of cases.

Its effect was to increase the force rather than the frequency of uterine contractions. Did it possess a strong tendency to produce miscarriages, there would have been but a very small natural increase of the population here in those times, but the reverse was the fact.

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Case of Bending of the Bones of the Forearm in a Child two years old. By W. C. EICHELBERGER, M.D., of Eugene, Ind.

Was called Sep. 3, 1871, to see a girl, about two years old, who, while playing, fell from a step about twelve inches in height, striking principally upon her right hand. Upon examination, found a hard projection, upon the dorsal surface of the right forearm, at the junction of the middle and lower thirds of arm; with a concavity on the palmar surface at point of injury; no crepitation or pain, pronation and supination but slightly interfered with. Upon rotating the arm so that the radial border became the upper one, it could be seen that there was a bending of both radius and ulna at the seat of injury. I diagnosed angular curvature of both bones; the data for the opinion being, absence of all of the usual signs of fracture in this region, no pain, slight interference with rotary action of arm, and age of the child.

The radius and ulna were bent at an angle of about eight degrees.

The injury was treated by placing a firm compress upon the projection, then applying long splints, as directed in fractures of the forearm, and over all a roller bandage, firmly applied, the apparatus reapplied whenever necessary, so as to keep up constant pressure upon the projection formed by the bending of the bones. The dressings were removed in four weeks from receipt of injury, when the limb was found to be restored to its normal condition. Any one not acquainted with the circumstances would be unable to tell which was the injured arm.

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Amenorrhœa relieved by the Hypodermic Use of Morphia and Atropia. By R. Z. LEEDS, M.D., of Hilliard, Ohio.

Miss J. F., æt. 18, has suffered from *suppressio mensium* at almost every period since puberty; often becoming delirious, biting her hands and raving in such a manner as to require force to restrain her from committing acts of violence. First menstruated at the age of 13; is very plethoric; weight, one hundred and seventy-five pounds; accustomed to much exercise. The menses occur regularly, but nearly always cease after the first day or two; when she is seized with rigours, followed by pyrexia each day, until the time of menstruation is past. Her mind has been affected seriously by this return each month. Believing that amenorrhœa is not a disease, but a symptom of some local or general disease, we have treated her accordingly with iron, quinia, strychnia, etc., without much benefit.

February 3d. Last night she had scarcely any sleep, but is more quiet

this morning. At three o'clock P.M., had appreciable rigors, followed by fever. I saw her at five o'clock; she was then unconscious and raving; pulse ninety-eight; respiration sixteen. I injected at the insertion of the deltoid eight minims of Dr. Bartholow's solution of morphia and atropia (morphia grs. xvi; atropia grs. ij; aqua dist. ℥j). Eight minims of this solution contains about one-thirtieth of a grain of sulphate of atropia and one-sixth of a grain of the sulphate of morphia. In fifteen minutes after the injection the patient became quiet. In thirty minutes was sleeping heavily. The pulse-rate began to rise, and in three-quarters of an hour from the administration it was one hundred and sixteen; at the same time respiration had fallen to twelve. Two hours from giving the injection, respiration and circulation had become nearly normal, the patient had a good night's sleep, and has had no return of the paroxysm since. In this case venesection would, I think, have been more effectual. By bleeding every month, between the menstrual epochs, I doubt not this periodical trouble would have been averted, but bloodletting is so undeservedly unpopular under any circumstances, that patients are not willing to submit to it.

Case of Triplets. By Dr. L. S. Murray, of Modena, Ohio.

I was called at 11 A.M., January 3, 1872, to attend Mrs. T., æt. 30 years, in her third confinement, and who was then in about the eighth month of pregnancy. The pains were normal, and in about half an hour after my arrival found, on examination, the sac ruptured and a head presentation, which was speedily delivered. I had scarcely time to tie the cord and remove the child, when a second sac of membranes presented at the vulva, which, being ruptured, the breech of a child presented and was expelled in about twenty minutes after the first. On passing the hand over the abdomen, the uterus was found still large, and pains continued, when the sac of a third child presented at the vulva, which I ruptured, and the feet presented, and was delivered in about half an hour after the second. The delivery of the three children being thus completed in about fifty minutes. All were females, weighing about three pounds apiece. There were three distinct placentæ, which were removed in thirty minutes after the birth of the last child. The uterus contracted readily, with but little hemorrhage. Patient's pulse sixty-eight, and of fair strength. About two months previous to delivery the mother had an attack of pneumonia, since which time she had been suffering from a severe cough and partial loss of voice, to which I attribute her premature labor. The cough yielded to treatment, and the mother made a complete recovery.

DOMESTIC SUMMARY.

Carbolic Acid as an Anæsthetic.—Dr. Andrew H. Smith reported to the Medical Society of the County of New York, April 22, 1872 (*New York Med. Journ.*, June, 1872) some experiments made upon himself which fully confirm the statement of Dr. J. H. Bill (see No. of this Journal for October, 1870) as to the local anæsthetic power of carbolic acid.

"In my first experiment," says Dr. Smith, "I painted a spot on the forearm, about an inch in diameter, with carbolic acid of about the strength of 85 per cent. For about a minute there was a slight burning sensation, after which

the integument became entirely insensible, the cuticle being whitened and shrivelled, and the spot slightly elevated. I then with a scalpel made an incision about half an inch in length through the whole thickness of the integument. This was done without even feeling the contact of the knife. The capillary circulation seemed not to be materially interfered with, as the blood flowed as freely as it would from a similar wound under ordinary circumstances. The reparative process was also not impaired, adhesion taking place immediately. Three hours after the application of the acid a needle could be thrust freely into the skin without causing pain.

"In the second experiment carbolic acid was applied as before, and ten minutes after a fly-blister was placed upon the spot. The blister remained eight and a half hours without causing any pain, and without producing vesication.

"In two instances I have applied the acid previous to incising a whitlow. The operation was almost painless, but, as the whitlow was in each case of the superficial variety, the test was not entirely conclusive.

"Inhaled in the form of spray, I have found the acid very useful in allaying irritation of the bronchial mucous membrane; coughs which have resisted all ordinary treatment have been immediately relieved, and in the course of two or three days entirely removed.

"I would suggest the use of a strong solution of carbolic acid as a revulsive, in cases in which a continuous impression is desired. While causing but little suffering, it produces an intense hyperæmia of the skin, which persists for eight or ten days, and is followed by desquamation of the cuticle."

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Quinia as an Oxytocic.—Dr. JAMES C. HARRIS expresses (*American Practitioner*, April, 1872) his disbelief that quinia originates contractions in the gravid uterus. He states that during the year 1839 he practised in Cedar Bluff, Ala., where during the autumn intermittent fever prevailed to a considerable extent. "In some of these cases," he says, "either from the severity or frequent recurrence of the chills, or both, the pain and aching in the lumbar and pelvic regions were so severe in several pregnant ladies as to produce abortion or premature labour. As cinchona or any of its salts could not at that time be secured in sufficient quantities at any price, I am certain that during this fall and the early part of the winter I attended as many as four cases of premature labour, the result alone of the above-mentioned causes, and heard of the occurrence of at least as many more well-authenticated similar cases in the hands of others. In two of the cases attended by me the children were born during the chill. Late in the fall, still having several ladies under my care suffering from chronic chills and fever, who were pregnant, and threatened at the recurrence of nearly every chill, from the lumbar and pelvic pains, with premature labour, I sent a messenger to Huntsville and purchased two or three ounces of quinia, with which, as soon as it arrived, properly conjoined with calomel or blue mass or morphia, as each particular case appeared to require, I went to work, and soon had the satisfaction to learn that their nausea, vomiting, lumbar and pelvic pains were relieved, and their chills and fever cured. Never since this time have I believed that quinia, as ordinarily administered, possessed any properties that were in the least calculated to produce contractions in the gravid uterus; but on the contrary, under certain circumstances, have always relied upon it as the very best remedy we know anything of to destroy the cause and break up the concatenation of symptoms upon which, among us, abortion frequently depends."

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Cæsarean Operations in the United Stases.—Dr. ROBERT P. HARRIS has, with much industry, culled reports of the Cæsarean operations performed in the United States, and gives (*Amer. Journ. Obstetrics*, Feb. 1872) the following table of these operations performed during or at the close of the first day of labour, showing the value of timely surgical interference. In all the cases but one the child was removed alive.

TABLE OF THE CÆSARRAN OPERATIONS IN THE UNITED STATES

Which were performed during or at the close of the first day of labour, showing the value of timely surgical interference. In all of the cases but one, the child was removed alive.

No.	Year.	Locality.	Age.	Colour.	Cause of difficulty.	Name of operator.	Result to woman.	Result to child.	Cause of death.	Hours in labour before operation.
1	1835	Philadelphia, Pa.	26	White	Deformed pelvis	Dr. Wm. Gibson ...	Recovered	Alive	.....	A few hours only.
2	1837	" "	28	"	" "	" " " ...	"	"	.....	" " "
3	1838	New York City.....	42	"	" "	" R. K. Hoffman	"	Dead	Child deformed, and died soon after delivery	Twenty-four hours.
4	1838	Near N. Orleans, La.	...	Black	Not any.....	Drunken midwife..	"	Alive	.....	A few hours.
5	1840	Hamilton, Ohio	...	White	Deformed pelvis	Dr. Cyrus Falconer	Died	"	Peritonitis.....	Less than 24 hours.
6	1846	Thibodeaux, La.....	30	Black	Exostosis.....	" J. A. Scudday	Recovered	Dead	Child feeble, and died soon after delivery	Twelve hours.
7	1849	Thibodeaux, La.....	33	"	"	" " "	"	Alive	.....	A few hours.
8	1849	Richmond, Miss.	...	"	Ventral hernia ..	" B. Harvey	Died in 3 weeks	"	Irritative fever.....	" "
9	1852	Wilcox County, Ala.	31	"	Impacted head ..	" Zacch. Nettles	Recovered	"	.....	Fifteen hours.
10	1854	Oktibbeha, Miss.	26	"	Deformed pelvis	" W. H. Merinar	"	"	.....	Before rupture of membranes.
11	1855	Oktibbeha, Miss.	27	"	" "	" " "	Died	"	Peritonitis.....	A few hours.
12	1856	Richmond, Va.....	23	"	" "	" Charles S. Mills	Recovered	"	.....	4½ hours actively.
13	1857	Council Bluffs, Iowa	20	White	" "	" W.F.McClelland	"	"	.....	Early.
14	1861	Hamburg, Ark.	...	Black	" "	" I. J. Newton....	"	"	.....	A few hours.
15	1863	Jefferson Co., Ind...	...	White	Fibrous tumour	" J. W. Conway..	"	Dead	Cause not given	Fourteen hours.
16	1869	Philadelphia, Pa. ...	31	"	Deformed pelvis	" Walter F. Atlee	Died.	Alive	Obstruction of bowels ...	A few hours.
17	1869	Ann Arbor, Mich. ...	35	"	" "	" Abram Sager...	"	"	Secondary shock	Eight or nine hours.

White women, 8 Recovered, 5 Children saved, 6
Black " 9 " 7 " " 8

$73\frac{10}{13}$ per cent. of women, and $86\frac{2}{13}$ of children saved by operating early.

Chloral Hydrate in Traumatic Tetanus.—Dr. Jos. R. BECK records (*St. Louis Med. and Surg. Journ.*, June, 1872) a case of traumatic tetanus in a railroad employee, to whom he gave chloral hydrate in doses of sixty grains every half hour until seven doses were taken, when he fell asleep and slept for thirty hours without stirring. When seen by Dr. B. half an hour after he awoke he said he felt as well as ever, with the exception of much muscular soreness in the spinal region and that of the backs of the thighs and legs, and some stiffness of the jaws. He had no subsequent spasm, and no further treatment was ordered for him. The slight stiffness wore off in two or three days; the after-effects of the chloral were *nil;* the wounds healed up rapidly; and to this day he has experienced no bad effects from his heroic treatment. He took four hundred and twenty grains of chloral in just three and a half hours.

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Monobromide of Camphor.—Prof. DEUEFFE, of Ghent, states (*Presse Med. Belge*) that he has been in the habit for more than two years of using this combination of bromine and camphor, and finds it an excellent sedative to the nervous system and in delirium tremens. Dr. WM. A. HAMMOND confirms (*New York Med. Journal*, May, 1872) this statement. He says, "My experience with the monobromide of camphor, though thus far limited, is eminently satisfactory. I have employed it in two cases of infantile convulsions due to the irritation of teething, with the effect in each instance of preventing the further occurrence of paroxysms which, previously to its administration, had been very frequent. In each case a grain was given every hour, rubbed up with a little mucilage of acacia. Three doses were sufficient in one, and two in the other case. The children were aged respectively fifteen and eighteen months.

"In a very obstinate case of hysteria occurring in a young married lady in the form of paroxysms of weeping and laughing, alternating with epileptiform and choreiform convulsions, I gave the monobromide of camphor in doses of four grains every hour. The influence was distinctly perceived after two doses were taken, but ten were necessary to entirely break up the attack. This was a very favorable result, as all previous seizures had lasted for from five to eleven days, uninfluenced by medication or moral suasion.

"I have also employed it with excellent effect in several cases of headache occurring in women and young girls, and due to mental excitement and excessive study. One dose of four grains was generally sufficient to cut short the attack. In two cases, three doses at intervals of half an hour were necessary.

"In wakefulness, the result as it so generally is of cerebral hyperæmia, the monobromide of camphor appears to be greatly inferior to the bromide of calcium or even the other bromides. But it is apparently indicated in delirium tremens. I have not yet had the opportunity of trying it in this disease, but I should not hesitate in a case of the affection to administer it in doses of five grains every hour or half-hour, with the confident expectation that sedation and sleep would result.

"The monobromide of camphor may be given in the form of pill, with conserve of roses as the excipient, or as a mixture with mucilage of gum-arabic and syrup. The dose for adults ranges from two to five grains."

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Recurrent Variola.—Dr. M. E. WEBB reports (*Boston Med. and Surg. Journal*, June 6, 1872) an interesting case of this in a healthy and well nourished child aged seven months, who a month previously had been unsuccessfully vaccinated. The disease was discrete variola, so diagnosed by competent authorities. The child, probably, was infected by the sister, and a fatal case of the disease died some days after. The child perfectly recovered, with several well marked scars on various parts of the body. Five weeks after the child had a second attack which proved fatal on the eighth day, during the secondary fever.

Dr. W. was informed by Dr. Green, City Physician, that he had seen during the present year seven cases in which variola had occurred a second time. In these, however, many years (from fifteen to forty) had elapsed between the first and second attacks, while in the case first cited, the disease occurred in the short space of five weeks.

UNIVERSITY OF PENNSYLVANIA.

MEDICAL DEPARTMENT.

ONE HUNDRED AND SEVENTH SESSION.

Ninth Street, above Chestnut, Philadelphia.

The Lectures of the Session of 1872–3 will commence on the First Monday (7th) of October, and close on the last day of February ensuing.

MEDICAL FACULTY.

GEORGE B. WOOD, M.D.,	Emeritus Professor of Theory and Practice of Medicine.
HUGH L. HODGE, M.D.,	Emeritus Professor of Obstetrics and the Diseases of Women and Children.
HENRY H. SMITH, M.D.,	Emeritus Professor of Surgery.
JOSEPH CARSON, M.D.,	Professor of Materia Medica and Pharmacy.
ROBERT E. ROGERS, M.D.,	Professor of Chemistry.
JOSEPH LEIDY, M.D.,	Professor of Anatomy.
FRANCIS G. SMITH, M.D.,	Professor of Institutes of Medicine.
R. A. F. PENROSE, M.D.,	Professor of Obstetrics and the Diseases of Women and Children.
ALFRED STILLÉ, M.D.,	Professor of Theory and Practice of Medicine, and of Clinical Medicine.
D. HAYES AGNEW, M.D.,	Professor of Surgery.
H. LENOX HODGE, M.D.,	Demonstrator of Anatomy.

Clinical Instruction is given daily throughout the year, in the Medical Hall, by the Professors and Clinical Lecturers and at the Hospitals. At the Philadelphia Hospital, and at the Pennsylvania Hospital, the instruction is free.

The Dissecting Rooms, under the superintendence of the Professor of Anatomy and the Demonstrator, are open from the first of September.

The room for Operative Surgery and the Application of Bandages, etc. etc., is open early in September and throughout the Session, under the supervision of the Professor of Surgery.

SUMMER MEDICAL ASSOCIATION OF THE UNIVERSITY.

Practical Chemistry	Prof. ROBERT E. ROGERS, M.D.
Physiology	Prof. FRANCIS G. SMITH, M.D.
The Cell Doctrine	Prof. JOSEPH LEIDY, M.D.
Regional Anatomy	H. LENOX HODGE, M.D.
Microscopy	JAMES TYSON, M.D.
Clinical Surgery	Prof. D. HAYES AGNEW, M.D.
Morbid Anatomy	JOSEPH G. RICHARDSON, M.D.
Clinical Medicine and Physical Diagnosis	WILLIAM PEPPER, M.D.
Diseases of Women and Children	WILLIAM GOODELL, M.D.
Surgical Diseases of the Mouth	JAMES E. GARRETSON, M.D.
Diseases of the Eye and Ear	WILLIAM F. NORRIS, M.D. and GEO. STRAWBRIDGE, M.D.
Diseases of the Skin	LOUIS A. DUHRING, M.D.
Diseases of the Nervous System	HORATIO C. WOOD, Jr., M.D.

Lectures on the above subjects are delivered by the members of the Summer Association annually during the months of April, May, and June, September, and the early part of October.

The lectures of this Preliminary Course will this year begin on Monday, September 2, and continue until the opening of the Regular Session. These Lectures are free to all matriculants of the University, upon registering their names with the Secretary of the Association, who will furnish them with tickets.

EXPENSES.—Fees for the Course of Lectures, $140. Matriculating Fee (paid once only), $5. Graduating Fee, $30.

AUXILIARY FACULTY OF MEDICINE.

HARRISON ALLEN, M.D., Professor of Zoology and Comparative Anatomy.
HORATIO C. WOOD, Jr., M.D., Professor of Botany.
FERDINAND V. HAYDEN, M.D., Professor of Mineralogy and Geology.
HENRY HARTSHORNE, M.D., Professor of Hygiene.
JOHN J. REESE, M.D., Professor of Medical Jurisprudence, including Toxicology.

The 8th Course of the Auxiliary Lectures will begin on the last Monday in March, and terminate the last Thursday in June. These lectures are free to all Students of the regular Medical Course.

R. E. ROGERS, M.D., *Dean of the Medical Faculty*, University Building.

W. H. SALVADOR, *Janitor*, University Building.

P. S.—Board may be had at from $5 00 to $6 00 per week.

*** *Alumni of this Department who are permanently settled, and other medical practitioners who desire to receive the Catalogue and Announcement regularly, are respectfully requested to send their addresses to the Dean, P. O. Box 2838, Philadelphia.*

HARVARD UNIVERSITY.

MEDICAL DEPARTMENT—BOSTON, MASS., 1872-73.

Changes in the Plan of Study and the Requisites for a Degree.

EIGHTY-NINTH ANNUAL ANNOUNCEMENT.

THE plan of study in this School has been radically changed. Instruction will hereafter be given by lectures, recitations, clinical teaching, and practical exercises uniformly distributed throughout the academic year. This year begins September 26th, and ends June 25th; it will be divided into two equal terms, with a recess of one week between them. Either of these two terms will be more than equivalent to the former "Winter Session," as regards the amount and character of the instruction.

The course of instruction has been greatly enlarged, so as to extend over three years, and has been so arranged as to carry the student progressively and systematically from one subject to another in a just and natural order.

In the subjects of anatomy, histology, chemistry, and pathological anatomy, laboratory work will be substituted for, or added to, the usual didactic lectures, and laboratory work will be as much required of every student as attendance at lectures and recitations.

Instead of the customary hasty, oral examination for the degree of Doctor of Medicine, held at the end of the three years' period of study, a series of examinations on all the main subjects of medical instruction has been distributed for regular students throughout the whole three years; but they may be passed by other students either all at once at the end of their course, or, successively, at several times. Every candidate for the degree must hereafter pass a satisfactory examination in every one of the principal departments of medical instruction at some time during his period of study. The regular examinations will be held at the end of each year in June, a week before the opening of the School in September, and at the close of the first term in February. The general subjects of the Regular Course of study are:—

For the first year—Anatomy, Physiology, and General Chemistry.

For the second year—Medical Chemistry, Materia Medica, Pathological Anatomy, Theory and Practice of Medicine, Clinical Medicine, Surgery and Clinical Surgery.

For the third year—Pathological Anatomy, Therapeutics, Obstetrics, Theory and Practice of Medicine, Clinical Medicine, Surgery and Clinical Surgery.

Students who take the regular course of the School will be divided into three classes according to their time of study and proficiency. Students may be admitted to advanced standing in the regular course; but all persons who apply for admission into the second or third year's class must pass an examination in the branches already pursued by the class to which they seek admission. Students who fail in any subject at one examination may be examined at the next examination. The regular examinations will be held in the following order:—

At the end of the first year—Anatomy, Physiology, and Chemistry.
" " " second year—Medical Chemistry, Materia Medica, and Pathological Anatomy.
" " " third year—Therapeutics, Obstetrics, Theory and Practice of Medicine, Clinical Medicine, Surgery, and Clinical Surgery.

Students who began their professional studies elsewhere may be admitted to the School and become candidates for a degree without joining the regular classes; such students may take up the subjects which they have not previously studied, in such order as may be thought best, passing the examinations at the beginning, middle, and end of each year.

Students who do not intend to offer themselves for a degree, may join the School for one term or more, and pay for instruction in such subjects as they select. Such students will be furnished, without examination, with certificates of attendance.

REQUIREMENTS FOR A DEGREE.—Every candidate must be twenty-one years of age; must have studied medicine three full years, have spent at least one continuous year at this School, have passed the required examinations, and have presented a thesis.

FEES.—For Matriculation, $5; for the Year, $200; for either Term, $120; for Graduation, $30; for courses in single subjects, according to the detailed announcement.

☞ The above supersedes the old plan of instruction, and went into operation on September 28, 1871.

A blue Catalogue containing detailed information will be sent on addressing

JAMES W. HARRIS, *Secretary*,
Cambridge, Mass.

For special information, address

Dr. C. ELLIS, *Dean*,
114 Boylston Street, Boston.

UNIVERSITY OF LOUISIANA—MEDICAL DEPARTMENT.

NEW ORLEANS.

FACULTY.

A. H. Cenas, M.D., Emeritus Professor of Obstetrics and Diseases of Women and Children.
James Jones, M.D., Professor of Obstetrics and Diseases of Women and Children.
T. G. Richardson, M.D., Professor of General and Clinical Surgery.
Samuel M. Bemiss, M.D., Professor of the Theory and Practice of Medicine and Clinical Medicine.
Stanford E. Chaillé, M.D., Professor of Physiology and Pathological Anatomy.
Frank Hawthorn, M.D., Professor of Materia Medica and Therapeutics, and Clinical Medicine.
Joseph Jones, M.D., Professor of Chemistry and Clinical Medicine.
Samuel Logan, M.D., Professor of Anatomy and Clinical Surgery.
E. Souchon, M.D., Demonstrator of Anatomy.

The next annual course of instruction in this Department (now in the thirty-eighth year of its existence), will commence on Monday, the 18th day of November, 1872, and terminate on the second Saturday of March, 1873. Preliminary Lectures on Clinical Medicine and Surgery will be delivered in the amphitheatre of the Charity Hospital, beginning on the 18th of October, without any charge to students. The Anatomical Rooms will be opened at the same time. The members of the Faculty are Visiting Physicians and Surgeons to the Charity Hospital, and give instruction daily *at the bedside* of the sick. The hospital received during the past year 6687 patients, and offers unsurpassed advantages for the practical study of medicine and surgery.

FEES.—For all the lectures, $140; Matriculation, $5; Practical Anatomy, $10; Graduation, $30. Payment required in advance.

For further information, address

T. G. RICHARDSON, M.D., *Dean.*

PRIVATE INSTRUCTION IN MEDICAL MICROSCOPY.

The applications of the Microscope in Medicine have of late years become so important, that its systematic employment in solving the problems of Diagnosis and *Prognosis* is almost a necessity, and well-educated communities will soon require of their physicians a constant resort to its manifestly invaluable revelations respecting disease.

Experience in teaching microscopy, both by lectures to classes and by direct private tuition, has so far demonstrated the superiority of the latter method, that the subscriber proposes to continue his course of instruction to students and graduates, throughout the year, in series of fifteen lessons each, arranged in such a manner as to avoid interference with College Lectures, or other engagements.

Fee (including Handbook) $15 00

For full particulars, address

JOSEPH G. RICHARDSON, M.D.,
(Lecturer on Pathological Anatomy in the University of Pennsylvania, and Microscopist to the Pennsylvania Hospital.)
No. 1620 Chestnut Street, Philadelphia.

UNIVERSITY OF MICHIGAN—MEDICAL DEPARTMENT.

The Lectures of the Session 1872-73 will commence on the first day of October, and continue for six months.

Besides the Clinical Lectures, four Diadactic Lectures will be delivered daily through the entire term.

Fees.—To students of Michigan, $20 for the first year; $10 for all subsequent years.

To all others, $35 for the first year, and $10 for all subsequent years.

For further information address,

ABM. SAGER, M.D.
Dean of the Medical Faculty, Ann Arbor, Michigan.

ALBANY MEDICAL COLLEGE.

TERM, 1872.

FACULTY.

JAMES McNAUGHTON, M.D., President; Theory and Practice of Medicine.
JAMES H. ARMSBY, M.D., Principles and Practice of Surgery and Clinical Surgery.
EDWARD R. PEASLEE, M.D., LL.D., Diseases of Women and Children.
WILLIAM P. SEYMOUR, M.D., Obstetrics.
MEREDITH CLYMER, M.D., Diseases of the Nervous System and of the Mind.
JOHN V. LANSING, M.D., Physiology and Clinical Medicine.
ALBERT VAN DERVEER, M.D., General and Special Anatomy.
HENRY R. HASKINS, M.D., Surgical and Descriptive Anatomy.
GEORGE T. STEVENS, M.D., Ophthalmic and Orthopedic Surgery.
JOHN M. BIGELOW, M.D., Materia Medica.
MAURICE PERKINS, A.M., M.D., Chemistry and Toxicology.
HON. IRA HARRIS, LL.D., President of the Board of Trustees; Medical Jurisprudence.
HENRY MARCH, M.D., Curator of Museum.

The College Term commences on the first Tuesday of September. SIX LECTURES are given daily—three Clinical Lectures each week in the hospital. The usual surgical clinique is held in the College each Saturday.

FEES for the Course, $100; Matriculation, $5; Graduation, $25; Perpetual Ticket, $150. For Circulars, apply to

J. V. LANSING, M.D., *Registrar.*

PHILADELPHIA SCHOOL OF ANATOMY,

Chant Street, Tenth Street above Chestnut, opposite the Mercantile Library.

COURSES OF LECTURES ON PRACTICAL SUBJECTS.

The following Courses of Lectures will be delivered in this Institution during each WINTER and SUMMER Session.

I. Anatomy	By Dr. W. W. KEEN.
II. Operative Surgery	" Dr. W. W. KEEN.
III. Bandaging, Fractures, and Fracture Dressings .	" Dr. O. H. ALLIS, 1005 Walnut Street.
IV. Physical Diagnosis	" Dr. O. P. REX, 100 North 15th Street.

The Course on ANATOMY begins the day after the Colleges open in October and in April, and consists of systematic Lectures, amply illustrated by the Class Microscope, Dissections, Models, &c.

The DISSECTING ROOMS are open all the year, except July and August, with a full supply of material.

Each of the other Courses begins one week later, and consists of practical demonstrations to the class, after which each member in turn is exercised in the various operations, applies the bandages or fracture dressings, or auscults and percusses the patients.

Fee for each Course . . . . . $10.

For further information, apply to the Janitor, at the Rooms, or to

W. W. KEEN, M.D.,
1729 Chestnut Street. (8½ to 5 P.M.)